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THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

CONDUCTED BY

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"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster
vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

VOL. XXXVII.

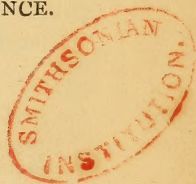
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JULY—DECEMBER, 1850.

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“Meditationis est perscrutari occulta; contemplationis est admirari perspicua Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condât,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonium.

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- III. Illustrative of Mr. A. J. Robertson's Paper on the Positive Wave of Translation.
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THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

JULY 1850.

I. *Second Memoir on the Magneto-optic Properties of Crystals, and the relation of Magnetism and Diamagnetism to Molecular Arrangement.* By JOHN TYNDALL and HERMANN KNOBLAUCH*.

IN the year 1846 our views of magnetic action received, through the researches of Faraday, an extraordinary expansion. The experiments of Brugmans, Le Baillif, Seebeck and Becquerel, had already proved the power to be active beyond the limits usually assigned to it; but these experiments were isolated, and limited in number. Faraday was the first to establish the broad fact, that there is no known body indifferent to magnetic influence, when the latter is strongly developed. The nature of magnetic action was then found to be twofold, attractive and repulsive; thus dividing bodies into two great classes, which are respectively denominated *magnetic* and *diamagnetic*.

The representative of the former class is *iron*, which, being brought before the single pole of a magnet, is attracted; the representative of the latter class is *bismuth*, which, being brought before the single pole of a magnet, is repelled.

If a little bar of iron be hung freely between the two poles of a magnet, it will set its longest dimension in the line joining the poles; a little bar of bismuth, on the contrary, will set its longest dimension at right angles to the line joining the poles.

The position of the iron is termed by Mr. Faraday the *axial*; the position of the bismuth, the *equatorial*. We shall have occasion to use these terms.

These discoveries, opening, as they did, a new field in physical science, invited the labours of scientific men on the continent. Weber, Ørsted, Reich and others, have occupied

* Communicated by the Authors.

Phil. Mag. S. 3. Vol. 37. No. 247. July 1850.

B

themselves with the subject. But, if we except the illustrious discoverer himself, there is no investigator in this branch of science whose labours have been, to all appearance, so richly rewarded as those of Professor Plücker of Bonn.

In 1847 M. Plücker had a magnet constructed of the same size and power as that described by Mr. Faraday*, his object being to investigate the influence of the fibrous constitution of plants upon their magnetic deportment. While conducting these experiments, he was induced to try whether crystalline structure exercised an influence. "The first experiment," says M. Plücker, "gave an immediate and decided reply."

Following up his investigations with crystals, he was led to the affirmation of the following two laws:—

"When any crystal whatever with one optical axis is brought between the poles of a magnet, the axis is repelled by each of the poles; and if the crystal possess two axes, each of these is repelled, with the same force, by the two poles."

"The force which causes this repulsion is independent of the magnetism or diamagnetism of the mass of the crystal; it decreases with the distance more slowly than the magnetic influence exerted by the poles†."

It is perhaps worth explaining, that if, on exciting the magnet, the optical axis take up the *axial* position, it is said to be attracted; if the *equatorial*, it is said to be repelled.

The first experiment of M. Plücker, which led to the affirmation of these laws, was made with tourmaline. A plate of the crystal which had been prepared for the purposes of polarization, twelve millimetres long, nine wide, and three thick, was suspended by a strong fibre between the poles of an electro-magnet. On sending a current round the latter, the plate, which was magnetic, set itself as an ordinary magnetic substance would do, with its longest dimension from pole to pole. The optical axis of the crystal, thus suspended, was vertical.

On hanging the crystal, however, with its optical axis horizontal; when the magnet was excited, the plate stood no longer as a magnetic substance, but as a diamagnetic; its longest dimension being at right angles to the line joining the poles. The optical axis of the crystal was found to coincide with its length, and the peculiar deportment was considered as a proof that the optical axis was repelled.

This law was further established by experiments with Iceland spar, quartz, zircon, beryl, &c., and, as above stated, included crystals of all kinds, both optic positive and negative.

* Phil. Mag., vol. xxviii. p. 396.

† Poggendorff's *Annalen*, vol. lxxii. p. 75.

It has, however, lately undergone considerable modification at the hands of M. Plücker himself. In a letter to Mr. Faraday, which appears in page 450, vol. xxxiv. of this Magazine, he expresses himself as follows:—

“The first and general law I deduced from my last experiments is the following one:—‘There will be *either repulsion or attraction* of the optic axes by the poles of a magnet, according to the crystalline structure of the crystal. If the crystal is a *negative* one, there will be *repulsion*; if it is a *positive* one, there will be *attraction**.’”

This law applies to crystals possessing two optical axes, each of the said axes being attracted or repelled according as the crystal is positive or negative. It will simplify the subject if we regard the line bisecting the acute angle inclosed by the two axes as the resultant of attraction or repulsion; for the sake of convenience, we shall call this the *middle line*. In positive crystals, therefore, the middle line, according to the above law, must stand *axial*; in negative crystals, *equatorial*. It is also evident, that the plane passing through the optical axes must, in the one class of crystals, stand from pole to pole, in the other class at right angles to the line joining the poles.

In explaining this new modification of the law, M. Plücker lays particular emphasis upon the fact, that the attraction or repulsion is the result of an independent force, connected in no way with the magnetism or diamagnetism of the mass of the crystal; and this view is shared by Mr. Faraday, who, in expressing his concurrence with M. Plücker, denominates the force in question an “*optic axis force*†.”

The experiments described in our first paper upon this subject‡, furnish, we conceive, sufficient ground of dissent from these views. In the case of five crystals of pure carbonate of lime (Iceland spar), we found the law of Plücker strictly verified, all five crystals being diamagnetic; on replacing, however, a portion of the carbonate of lime by carbonate of iron, nature herself being the chemist in this case, the crystal was no longer diamagnetic, but magnetic; in every other respect it was physically unchanged; its optical properties remained precisely as before, the crystal of carbonate of lime and the crystal of carbonate of lime and iron being both negative. In the one case, however, the optical axis was attracted; in the other, the said axis was repelled; the attraction being evidently caused by the passage of the crystal from the diamagnetic into the magnetic state.

* Phil. Mag., vol. xxxiv. p. 450.

† Phil. Trans. 1849, p. 32.

‡ Phil. Mag., vol. xxxvi. p. 178.

We have examined other crystals of the same form as Iceland spar, both magnetic and diamagnetic. In all cases, the former act in a manner precisely similar to the magnetic crystal already described, while the latter behave as the diamagnetic. The following are examples.

Nitrate of Soda.—This crystal is exactly of the same form as carbonate of lime, and, like it, diamagnetic. Its deportment is in every respect the same. A rhombus cloven from the crystal and suspended horizontally between the poles, sets its longer diagonal axial. Suspending the full crystal between the poles, with its optical axis horizontal, on exciting the magnet this axis sets itself equatorial.

Breunnerite.—This is a crystal composed principally of carbonate of lime and carbonate of magnesia, but containing a sufficient quantity of the carbonate of iron to render it magnetic. Suspended in the magnetic field, the optical axis stands from pole to pole.

Dolomite.—In this crystal a portion of the lime is replaced by protoxide of iron and protoxide of manganese, which ingredients render it magnetic. The optical axis stands from pole to pole.

Carbonate of Iron.—In the cases cited, the substitution of iron for calcium was partial; in the case before us it is complete. This crystal differs in nothing, save in the energy of its action, from the magnetic crystals already described. If a full crystal be hung between the poles, with its optical axis horizontal, on closing the circuit and sending a current round the magnet, the said axis turns strongly into the axial line, vibrates through it quickly for a time, and finally comes to rest there. If a thin rhombus be cloven from the crystal and suspended from one of its obtuse angles with its parallel faces vertical, it will set itself exactly equatorial. In this case it is easy to see that the horizontal projection of the optical axis, which passes through the obtuse angle of the crystal, stands axial. Hung from its acute angle, the rhombus takes up an oblique position, making a constant angle with the line joining the poles. To this position, if forcibly removed from it, it will invariably return. The position may be either right or left of the axial line; but the angle of obliquity is always the same, being the angle which the optical axis makes with the face of the rhombus. Hung from the obtuse angle the obliquity is nothing—from the acute angle it is a maximum; the rhombus is capable of all degrees of obliquity between these extremes, *the optical axis in all cases standing exactly from pole to pole.*

Oxide of Iron.—The above phænomena are exhibited even

in a more striking manner by this crystal. So strong is the directive power, that a rhombus, suspended from one of its obtuse angles, will set itself strongly equatorial, though its length may be fifteen or twenty times its breadth.

What is the conclusion to be drawn from these experiments? We have first of all a diamagnetic crystal of pure carbonate of lime, which sets its optical axis equatorial. On substituting for a portion of the lime a quantity of protoxide of iron sufficient to render the crystal weakly magnetic, we find the position of the axis at once reversed. Replacing a still further quantity of the diamagnetic lime by a magnetic constituent, we find the action stronger, the force with which the optical axis takes up the axial position increasing as the magnetic constituents increase. These experiments appear to be irreconcilable with the statement, that the position of the optical axis is independent of the magnetism or diamagnetism of the mass.

Turning now to crystals possessing two optical axes, we find the law of Plücker equally untenable; a forcible contradiction is exhibited in the case of—

Dichroite.—This crystal, as is well known, receives its name from its ability to transmit light of different colours. The specimen examined by us is a cube. In the direction of the crystallographic axis, which coincides with the *middle line*, the light transmitted is yellowish; through the other four sides of the cube it is a deep blue. Suspended with the middle line horizontal, whatever be the position of that line before closing the circuit, the instant the magnetic force is developed it turns with surprising energy into the axial position and becomes fixed there. According to the law, however, the middle line should stand equatorial, for the crystal is negative*.

Sulphate of Barytes (heavy spar).—The form of this crystal is a prism whose base is a rhombus, the four sides being perpendicular to the base. It cleaves parallel to the sides and base. Suspended between the poles, with the axis of the prism vertical, on exciting the magnet, the long diagonal sets itself axial. The crystal is diamagnetic, and agrees, thus far, with the carbonate of lime. Suspended from the acute angle formed by two sides of the prism, the axis of the latter being horizontal, on closing the circuit the said axis turns into the *axial* position, and remains there as long as the force is present. Suspending the crystal from its obtuse angle, the axis being still horizontal, on closing the circuit the axis sets itself *equatorial*. A plane perpendicular to the rhombic base, and passing through the long diagonal, contains the two optical

* Brewster's list.

axes, which are inclined to each other at an angle of 38° . The middle line bisecting this angle is parallel to the axis of the prism, and hence stands *axial* or *equatorial*, according as the prism is suspended from its acute or its obtuse angle. The position of the middle line is therefore a function of the point of suspension, varying as it varies; at one time supporting the law of Plücker, and at another time contradicting it. Heavy spar is positive.

Sulphate of Strontia (Cælestine).—This is also a positive crystal, its form being precisely that of heavy spar; the only difference is, that, in the case before us, the optic axes inclose an angle of 50° instead of 38° . The corroboration and contradiction of heavy spar are exhibited here also.

Sulphate of Zinc.—Suspend the crystalline prism from its end, and mark the line which stands equatorial when the magnet is excited. A plate taken from the crystal parallel to this line, and to the axis of the prism, on examination with polarized light, will display the ring systems surrounding the ends of the two optic axes. The middle line, therefore, which bisects the acute angle inclosed by these, stands axial. It ought, however, to stand equatorial, for the crystal is negative.

Sulphate of Magnesia.—Suspending the crystalline prism from its end, and following the method applied in the case of sulphate of zinc, we discover the ring systems and the position of the middle line. This line stands axial, but the crystal is negative.

Topaz.—This being one of the crystals pronounced by M. Plücker as peculiarly suited to the illustration of his new law, it is perhaps on that account deserving of more than ordinary attention. In the letter to Mr. Faraday, before alluded to, M. Plücker writes:—

“The crystals most fitted to give the evidence of this law are *diopside* (a positive crystal), *cyanite*, *topaz* (both negative), and others crystallizing in a similar way. In these crystals the line (A) bisecting the acute angles made by the two optic axes, is neither perpendicular nor parallel to the axis (B) of the prism. Such a prism, suspended horizontally, will point neither axially nor equatorially, but will take always a fixed intermediate direction. This direction will continually change if the prism be turned round its own axis (B). It may be proved by a simple geometrical construction, which shows that during one revolution of the prism round its axis (B), this axis, without passing out of two fixed limits C and D, will go through all intermediate positions. The directions C and D, where the crystal returns, make, *either* with the line joining the two poles, *or* with the line perpendicular to it, on

both sides of these lines, angles equal to the angle included by A and B; the first being the case if the crystal be a *positive* one, the last if a *negative* one. Thence it follows, that if the crystal, by any kind of horizontal suspension, should point to the poles of a magnet, it is a *positive* one; if it should point equatorially, it is a *negative* one*."

In experimenting with this crystal, we have found the greatest care to be necessary. Its diamagnetic force is so weak, that the slightest local impurity, contracted by handling or otherwise, is sufficient to derange its action. The crystals as they come from the mineralogist are unfit for exact experiment. We have found it necessary to boil those which we have used in muriatic acid, and to scour them afterwards with fine white sand, reduced to powder in a mortar. These precautions taken, we have been unable to obtain the results described by M. Plücker. We have examined five specimens of topaz from Saxony, the axial dimension of some of them exceeding the dimension perpendicular thereto by one-half; the axis, notwithstanding, stands in all cases from pole to pole. Two specimens of Brazilian topaz, the one of an amber colour, the other almost as clear as distilled water, give the same results; the axes of the crystals stand from pole to pole, and turning round makes no difference. On a first examination, some of the crystals exhibited an action similar to that described by M. Plücker; after boiling and scouring, these irregularities disappeared, and they one and all stood axial.

One crystal in particular caused us considerable embarrassment. Its action was irregular, and the irregularity remained after the adoption of the methods described to ensure purity. A splinter from one of its sides was found to be attracted, a splinter from the side opposite was found to be repelled. To the naked eye the crystal appeared clean and clear. On examination, however, under a powerful microscope, the side of the crystal from which the magnetic splinter was taken was found dotted with small black particles imbedded in its mass; the other side of the crystal was perfectly transparent. On cleaving away the impurities, the irregularity vanished, and the crystal stood as the others.

In the letter quoted, diopside is pronounced by M. Plücker to be a positive crystal. On examination with circular polarized light, as recommended by Dove†, we find the crystal to be negative. The same method pronounces topaz positive, instead of negative, as affirmed by M. Plücker. The specimens we have examined in this way are from Brazil and

* Phil. Mag., vol. xxxiv. p. 450.

† Poggendorff's *Annalen*, vol. xl. pp. 457, 482.

Saxony. Aberdeen topaz we have not examined, but it also is classed by Brewster among positive crystals. The obliquity of the middle line of topaz does not exist in the specimens which have come under our notice; it is exactly perpendicular to the principal cleavage, and consequently exactly parallel to the axis of the prism. This agrees with the results of Brewster, who found the optical axes to be "equally inclined to the plane of cleavage*."

In experimenting with weak diamagnetic crystals, the greater the number of examples the better; as, if local impurity be present, it is thus more liable to detection. Our results with heavy spar have been confirmed by ten different crystals; with coëlestine, by five; and with topaz, as has been stated, by seven. The suspending fibre, in these and similar instances, was a foot long and $\frac{1}{2500}$ of an inch thick, or about one-eighth of the diameter of a human hair.

Sugar.—It is well known that this crystal forms a prism with six sides, two of which are generally very prominent, the principal cleavage being parallel to these two, and to the wedge-like edge which runs along the end of the prism. The plane of the optical axes is perpendicular to the axis of the prism, and their ends may be found by cutting out a plate parallel to that axis, and inclined to the principal cleavage at an angle of about 20° . Such a plate exhibits both ring systems symmetrically, while a plate parallel to the principal cleavage exhibits one system only. Suspended between the excited poles, with the axis of the prism horizontal, and the principal cleavage vertical, the plane of the optical axis stands axial; according to the law of M. Plücker, it ought to stand equatorial, for the crystal is negative.

Rock-crystal (Quartz).—This crystal has undergone more than one examination by M. Plücker, its deportment being, "contrary to all expectation," very weak—a result, it may be remarked, difficult of explanation on the hypothesis of an "optic axis force." M. Plücker's first experiments with this crystal were apparently made with great exactitude, the crystal being reduced to a spherical shape, and the influence of mere form thus annulled. These experiments proved the optical axis to be repelled. Later researches, however, induced this philosopher to alter his opinion, and accordingly, in his last memoir†, we find quartz ranked with those crystals whose optical axes are attracted, with the remark "weak" added parenthetically. We have not been able to obtain this

* Lardner's Encyclopædia, Optics, p. 204.

† Poggendorff's *Annalen*, vol. lxxviii. p. 428.

deportment. After the washing and scouring process, the finest and most transparent crystals we could procure confirmed the first experiments of M. Plücker, and therefore contradict the new modification of his law. It is almost incredible how slight an impurity is sufficient to disturb the action of this crystal. A specimen with smaller crystals attached to it, or growing through it, is suspicious and ought to be rejected. Clear isolated crystals are alone suitable. We must remark that a fine cube, with faces half an inch square, suspended with the optic axis horizontal, showed no directive action; either one or the other of the diagonals set itself from pole to pole, though the axis ran parallel to four of the faces.

As far as it has been practicable, we have cut and cloven, and examined the optical properties of the crystals which have passed through our hands ourselves, testing, in every possible case, the results of others by actual experiment. Most of the crystals in Brewster's list have been gone through in this way. Iceland spar, quartz, mica, arragonite, diopside, lepidolite, topaz, saltpetre, sugar, sulphate of zinc, sulphate of magnesia, and others have been examined and verified. In two cases, however, our results differed from the list, these being sulphate of nickel and borax. A prism of sulphate of nickel was suspended from its end between the poles; on exciting the magnet it took up a determinate position. When it came to rest, a line parallel to the magnetic axis was marked thereon, and a plate taken from the crystal parallel to this line and to the axis of the prism. Such a plate, ground thin, exhibited in the polariscope a pair of very beautiful ring systems. The ring systems of borax were found in a similar manner. The middle line, therefore, in both cases stood equatorial, and, according to the list, would contradict the law of M. Plücker, for both are there set down as *positive*. A careful examination with circular polarized light led us to the opposite conclusion. We thought it worth while to send specimens of each to Berlin, so as to have them examined by Professor Dove, the author of the method by which we examined them. The crystals have been returned to us with a note certifying that they are *negative*, and thus confirming our observations. This certificate has reached us in the form of a private note, but we believe Professor Dove will not charge us with imprudence for thus availing ourselves of such a high authority as his opinion confessedly is in optical matters.

Yellow Ferrocyanide of Potassium.—This crystal does not stand in the list of Brewster, and we have sought for it in

other lists in vain. In one German work on physics we find *Blutlaugensalz* set down as a negative crystal with one optic axis, but whether the red or yellow salt is meant, the text does not explain. We have examined the crystal and find it positive with two optic axes. The magnetism stands perpendicular to the principal cleavage. Susceptibility with this line horizontal, on closing the circuit it sets up an equatorial. Another exception to the law under consideration is here exhibited.

M. Plücker recommends the magnet as a practical means of determining whether a crystal is positive or negative; this method being attended with the peculiar advantage that it can be applied in the case of opaque crystals, where all the ordinary methods fail. We find accordingly, in his last memoir on this subject, metallic and other opaque crystals with optical properties attributed to them. Antimony is negative with one optic axis; bismuth and arsenic are positive with one optic axis. The foregoing experiments demonstrate the insecurity of the basis on which this classification rests.

By looking back upon the results described, it will be seen that we have drawn from each respective class of crystals one or more examples which disobey the law of M. Plücker. Of positive crystals with one axis, we have quartz; of positive crystals with two axes, we have heavy spar, cœlestine and ferrocyanide of potassium. Of negative crystals with one axis, we have carbonate of lime and iron, and several others; of negative crystals with two axes, we have dichroite, sugar, sulphate of zinc, and sulphate of magnesia. It is due however to M. Plücker to state, that in a considerable number of cases we have found the law confirmed. Tourmaline, idocrase, beryl, Iceland spar, saltpetre, arragonite, and many others all confirm it. Singularly enough, these are the very crystals with which M. Plücker has experimented. It is therefore not to be wondered at, that he should be led by such a mass of concurring evidence to pronounce his law general. Had his experiments embraced a sufficient number of cases, they would doubtless have led him to the same conclusion to which ours have conducted.

Mr. Faraday has devoted considerable time to the investigation of this intricate subject. His most notable experiments are those with *bismuth*, *antimony*, *arsenic*, *sulphate of iron*, and *sulphate of nickel*, which experiments we have carefully repeated.

Bismuth.—Crystals of bismuth we have ourselves prepared, by melting the metal in a Hessian crucible, placed within a larger one and surrounded by fine sand. In this state it was

allowed to cool slowly, until a thin crust gathered on the surface. At this point the crust was pierced, and the molten metal underneath poured out, thus leaving the complete crystalline mass growing round the sides and bottom. Our experiments with these crystals corroborate, to the letter, those so fully described by Mr. Faraday in the Bakerian Lecture of 1849, delivered before the Royal Society*.

Arsenic.—Our arsenic we obtained at the druggists. It is well known that this metal is usually obtained by the sublimation of its ore, the vapour being condensed in suitable receivers, where it is deposited in a crystalline form. There is a difference of opinion between Mr. Faraday and M. Plücker regarding this metal; the former holding it for diamagnetic, the latter for magnetic. Several specimens, obtained from different druggists, corroborated the view of M. Plücker. They were all *magnetic*.

About half an ounce of the metal was introduced into a glass tube closed at one end and open at the other. About six inches of the tube, near the open end, was crammed full of copper turnings, and the open end introduced through a small aperture into the strong draft of a flue from a heated oven. The portion of the tube containing the copper turnings was heated to redness, and by degrees the oxygen within the tube was absorbed. The arsenic at the other end was then heated and sublimed. After some time the vapour was allowed to condense slowly, and a metallic deposit was the consequence—the arsenic thus obtained was *diamagnetic*. The deportment of the crystal is described by Mr. Faraday in the place referred to.

Antimony.—A difference of opinion exists with regard to the action of this crystal also. Referring to the deportment assigned to it by Mr. Faraday, M. Plücker writes, “to my astonishment however antimony behaved in a manner directly the reverse. While on the one side a prism of bismuth, whose principal cleavage coincided with the base of the prism, set itself *axial*; and on the other side a plate of arsenic, which, on account of its magnetism, ought to stand *axial*, set itself *equatorial*; a plate of antimony deviated completely from this deportment, and although the mass was strongly diamagnetic, set itself decidedly *axial*.”

M. Plücker's results differ from those of Mr. Faraday in two particulars; first, a plate of antimony, similar to that described by M. Plücker, is found by Mr. Faraday to stand *equatorial* instead of *axial*; second, the following phenomena, observed by Mr. Faraday, appear not to have exhibited

* Philosophical Transactions, 1849, p. 1.

themselves in M. Plücker's experiments:—"On the development of the magnetic force, the crystal went up to its position slowly, and pointed as with a dead set. Other crystals did the same imperfectly; and others again made one or perhaps two vibrations, but all appeared as if they were moving in a thick fluid, and were, in that respect, utterly unlike bismuth, in the freedom and mobility with which it vibrated. If the crystalline mass was revolving when the magnetic force was excited, it suddenly stopped, and was caught in a position which might, as was found by experience, be any position. The arrest was followed by a revulsive action on the discontinuance of the electric current*."

In most of the specimens examined by us these phænomena were also absent, and the results of M. Plücker presented themselves. Three specimens however behaved exactly in the manner described by Mr. Faraday, exhibiting a singular inertness when the magnetic force was present, and a revulsion from the poles on breaking the circuit. To ascertain, if possible, the cause of this difference, we dissolved an example of each class in muriatic acid, precipitated the antimony with distilled water, and tested the clear filtrate with ferrocyanide of potassium. The specimen which agreed with M. Plücker exhibited a faint bluish tint, characteristic of the presence of iron; that which corroborated Mr. Faraday showed not the slightest trace of this metal. The iron, though thus revealing itself, must have been present in a quantity exceedingly minute, for the antimony was diamagnetic. Whether this has been the cause of the difference between M. Plücker and Mr. Faraday we will not undertake to say; irregular crystalline structure may also have had an influence.

We have here a crowd of examples of crystalline action in the magnetic field, but as yet not a word of explanation. M. Plücker's hypothesis has evidently failed. We now turn to the observations of Mr. Faraday, and shall endeavour to exhibit, in the briefest manner possible, the views of this profound investigator.

After a general description of the action of bismuth between the poles, Mr. Faraday writes:—"The results are, altogether, very different from those produced by diamagnetic action. They are equally distinct from those dependent on ordinary magnetic action. They are also distinct from those discovered and described by Plücker, in his beautiful researches into the relation of the optic axis to magnetic action; for there the force is equatorial, whereas here it is axial. So they appear

* Philosophical Transactions, 1849, p. 14. For an explanation see Philosophical Magazine, vol. xxviii. p. 460.

to present to us a new force, or a new form of force in the molecules of matter, which for convenience sake, I will conventionally designate by a new word, as the *magnecrystallic force**."

"The magnecrystallic force appears to be very clearly distinguished from either the magnetic or diamagnetic forces, in that it causes neither approach nor recession; consisting not in attraction or repulsion, but in its giving a certain determinate position to the mass under its influence, so that a given line in relation to the mass is brought by it into a given relation with the direction of the external magnetic power†."

The line through the crystal which sets itself with greatest force from pole to pole, is termed by Mr. Faraday the magnecrystallic axis of the crystal. He proves by experiment that bismuth has exactly the same amount of repulsion whether this axis be *parallel* or *transverse* to the lines of magnetic force acting on it.

"In other experiments a vertical axis was constructed of cocoon silk, and the body to be examined was attached to it at right angles as radius; a prismatic crystal of sulphate of iron, for instance, whose length was four times its breadth, was fixed on the axis with its length as radius and its magnecrystallic axis horizontal, and therefore as tangent; then, when this crystal was at rest under the torsion force of the silken axis, an electro-magnetic pole was so placed that the axial line of magnetic force should be, when exerted, oblique to both the length and the magnecrystallic axis of the crystal; and the consequence was, that, when the electric current circulated round the magnet, the crystal actually *receded* from the magnet under the influence of the force, which tended to place the magnecrystallic axis and the magnetic axis parallel. Employing a crystal or plate of bismuth, that body could be made to *approach* the magnetic pole under the influence of the magnecrystallic force; and this force is so strong as to counteract either the tendency of the magnetic body to approach or of the diamagnetic body to retreat, when it is exerted in the contrary direction. Hence Mr. Faraday concludes that it is neither attraction nor repulsion which causes the set or determines the final position of a magnecrystallic body‡."

"As made manifest by the phænomena, the magnecrystallic force is a force acting at a distance, for the crystal is moved by the magnet at a distance, and the crystal can also move the magnet at a distance." Mr. Faraday obtained the latter result by converting a steel bodkin into a magnet, and suspend-

* Phil. Trans., 1849, p. 4.

† Phil. Trans., 1849, p. 22.

‡ Phil. Mag. vol. xxxiv. p. 77.

ing it freely in the neighbourhood of the crystal. The tendency of the needle was always to place itself parallel to the magnecrystallic axis.

Crystals of bismuth lost their power of pointing at the moment the metal began to fuse into drops over a spirit-lamp or in an oil-bath. "Crystals of antimony lost their magnecrystallic power below a dull red heat, and just as they were softening so as to take the impression of the copper loop in which they were hung." Iceland spar and tourmaline, on the contrary, on being raised to the highest temperature which a spirit-lamp could give, underwent no diminution of force; they pointed equally well as before.

Mr. Faraday finally divides the forces belonging to crystals into two—*inherent* and *induced*. An example of the former is the force by which a crystal modifies a ray of light which passes through its mass; the second is developed exclusively by magnetic power. To this latter, as distinct from the other, Mr. Faraday has given the name *magnetocrystallic*. To account for crystalline action in the magnetic field, we have, therefore, the existence of *three new forces* assumed:—the *optic axis* force, the *magnecrystallic* force, and the *magneto-crystallic* force.

With regard to the experimental portion of Mr. Faraday's labours on this subject, we have only to express our admiration of the perfect exactitude with which the results are given. It appears to us, however, a matter of exceeding difficulty to obtain a clear notion of any such force as he has described; that is to say, a force proceeding from the pole of a magnet, and capable of producing such motions in the magnetic field, and yet neither attractive nor repulsive.

That a crystal of bismuth should approach the magnetic pole, and that a crystal of sulphate of iron should recede therefrom, appears, at first sight, anomalous, but certainly not more so than other phænomena connected with one of Mr. Faraday's most celebrated discoveries, and explained in a beautiful and satisfactory manner by himself.

If we hang a penny from its edge in the magnetic field, and so arrange the suspending thread, that the coin, before the magnetic power is developed, shall make an angle of 45° , or thereabouts, with the line joining the poles; then, on closing the circuit, and sending a current round the magnet, the coin will suddenly turn, as if it made an effort to set itself from pole to pole; and if its position beforehand be nearly axial, this effort will be sufficient to set it *exactly* so; the penny thus behaving, to all appearance, as if it were attracted by the poles.

The real cause of this however is *repulsion*. During the

development of magnetic power, an electric stream is aroused in the copper coin, which circulates round the coin in a direction opposite to that of the current which passes from the battery round the coils of the magnet. The effect of this induced stream is to create a polar axis in the copper; and when the direction of the stream is considered, it is easy to see that the north end of this axis must *face* the north pole of the magnet, and will consequently be repelled. On looking therefore at the penny, apparently attracted as above described, we must, if we would conceive rightly of the matter, withdraw our attention from the coin itself, and fix it on a line passing through its centre, and at right angles to its flat surface; this is the polar axis of the penny, the repulsion of which causes the apparent attraction.

We do not mean to say that any such action as that here described takes place with a bismuth crystal in the magnetic field. The case is cited merely to show that the "approach" of the bismuth crystal, noticed by Mr. Faraday, *may*, be really due to *repulsion*; and the "recession" of the sulphate of iron really due to *attraction*.

Our meaning will perhaps unfold itself more clearly as we proceed. If we take a slice of apple, about the same size as the penny, but somewhat thicker, and pierce it through with short bits of iron wire, in a direction perpendicular to its flat surface, such a disc, suspended in the magnetic field, will, on the evolution of the magnetic force, recede from the poles and set itself strongly equatorial; *not* by repulsion, but by the attraction of the iron wires passing through it. If, instead of iron, we use bismuth wire, the disc, on exciting the magnet, will turn into the axial position; *not* by attraction, but by the repulsion of the bismuth wires passing through it.

If we suppose the slice of apple to be replaced by a little cake made of a mixture of flour and iron filings, the bits of wire running through this will assert their predominance as before; for though the whole is strongly magnetic, the superior energy of action along the wire will determine the position of the mass. If the bismuth wire, instead of piercing the apple, pierce a little cake made of flour and bismuth filings, the cake will stand between the poles as the apple stood; for though the whole is diamagnetic, the stronger action along the wire will be the ruling agency as regards position.

Is it not possible to conceive an arrangement among the particles of a magnetic or diamagnetic body, capable of producing a visible result similar to that here described? If the magnetic and diamagnetic forces be associated with the particles of matter, is it not a reasonable inference, that the closer

these particles are aggregated, the less will be the obstruction offered to the transmission of the respective forces among them? It is this closeness of arrangement in the cases just cited, which gives to the iron and bismuth wire their predominance; for the interposed flour particles obstruct the forces in all other directions. If, therefore, in a magnetic or diamagnetic mass, two directions exist, in one of which the contact of the particles is closer than in the other, may we not fairly conclude that the strongest exhibition of force will be in the former line, which therefore will signalize itself between the poles, in a manner similar to the bismuth or iron wire? The case seems analogous to that of good and bad conductors in electricity. This fluid will not quit the good conductor to go to the bad. The powder magazine is safe, because the fluid prefers the iron rod to any other path. As regards magnetism, different directions, *through the same body*, may represent these good and bad conductors; *the line of preference being that of closest contact among the material particles.*

If analogic proof be of any value, we have it here of the very strongest description. For example:—bismuth is a brittle metal and can readily be reduced to a fine powder in a mortar. Let a teaspoonful of the powdered metal be wetted with gum-water, kneaded into a paste, and made into a little roll, say an inch long and a quarter of an inch across. Hung between the excited poles, it will set itself like a little bar of bismuth—equatorial. Place the roll, protected by bits of paste-board, within the jaws of a vice, squeeze it flat, and suspend the plate thus formed between the poles. On exciting the magnet the plate will turn, with the energy of a magnetic substance, into the axial position, though its length may be ten times its breadth.

Pound a piece of carbonate of iron into a fine powder, and form it into a roll in the manner described. Hung between the excited poles, it will stand as an ordinary magnetic substance—axial. Squeeze it in the vice and suspend it edge-ways, its position will be immediately reversed. On the development of the magnetic force, the plate thus formed will recoil from the poles, *as if violently repelled*, and take up the equatorial position.

We have here “approach” and “recession,” but the cause is evident. The line of closest contact is perpendicular in each case to the surface of the plate—a consequence of the pressure which the particles have undergone in this direction; and this perpendicular stands axial or equatorial according as the plate is magnetic or diamagnetic. We have here a “a directive force,” but it is attraction or repulsion modified. May

not that which has been here effected by artificial means occur naturally? *Must* it not actually occur in most instances? for, where perfect homogeneity of mass does not exist, there will always be a preference shown by the forces for some particular direction. This election of a certain line is therefore the rule and not the exception. It will assist both the reader and us if we give this line a name; we therefore propose to call it *the line of elective polarity*. In magnetic bodies this line will stand axial, in diamagnetic equatorial.

“The relation of the magnecrystallic force,” says Mr. Faraday, “to the magnetic field is axial and not equatorial.” This he considers to be proved by the following considerations:—suppose a crystal of bismuth so suspended that it sets with its *maximum* degree of force, then if the point of suspension be moved 90° in the axial plane, so that the line which in the last case stood horizontal and axial, may now hang vertical, then the action is a *minimum*: now, contends Mr. Faraday, if the force were equatorial this change in the axial plane ought not to have affected it; that is to say, if the force act at right angles to the axial plane, it is all the same which point of the plane is chosen as the point of suspension.

This seems a fair conclusion; but the other is just as fair—that, if the force be axial, a change of the point of suspension in the equatorial plane cannot disturb it. Mr. Faraday finds the line of maximum force in sulphate of nickel to be parallel to the axis of the prism. Whatever, therefore, be the point of suspension in the plane perpendicular to the axis, the action ought to be the same. On examining this crystal it will probably be found that two opposite corners of the parallelopiped are a little flattened. Let the prism be hung with its axis horizontal and this *flattening* vertical, and after the evolution of the magnetic force let the oscillations of the prism be counted. Move the point of suspension 90° in the equatorial plane, so that the flattening shall be horizontal, and again count the oscillations. The numbers expressing the oscillations in both cases will be very different. The former will be a *maximum*, the latter a *minimum*. But if the force be axial this is impossible, therefore the force is not axial.

Whatever be the degree of conclusiveness which attaches itself to the reasoning of Mr. Faraday drawn from bismuth; precisely the same degree attaches to the above drawn from sulphate of nickel. The conclusions are equal and opposite, and hence destroy each other. It will probably be found that the reasoning in both cases is entirely correct; that the force is neither axial nor equatorial, in the sense in which these terms are used.

A number of thin plates, each about half an inch square, were cut from almond kernels, with an ivory blade, parallel to the cleft which divides the kernel into two lobes. These were laid one upon the other, with an interval of strong gum between, until a cube was obtained. A few minutes in the sunshine sufficed to render the cube dry enough for experiment. Hung between the poles, with the line perpendicular to the layers horizontal, on exciting the magnet this line turned and set itself parallel to the magnetic resultant passing through the mass. The action here was a *maximum*. Turning the cube round 90° in the axial plane, there was scarcely any directive action. If the word 'crystal' be substituted for 'cube' in the description of this deportment, every syllable of it is applicable to the case of bismuth; and if the deportment of the crystal warrants the conclusion that the force is axial, the deportment of the cube warrants the same conclusion. *Is the force axial in the case of the cube? Is the position of the line perpendicular to its layers due to the "tendency" of that line to set itself parallel to the magnetic resultant? The kernel is strongly diamagnetic, and the position of the perpendicular is evidently a secondary result, brought about by the repulsion of the layers. Is it not then possible, that the approach of the magnecrystalline axis, in bismuth, to the magnetic resultant, is really due to the repulsion of the planes of cleavage?*

But here the experiment with the silken axis meets us; which showed that so far from attraction being the cause of action in a magnetic crystal, there was actual *recession*; and so far from repulsion being the cause in a diamagnetic crystal, there was actual *approach*. This objection it is our duty to answer.

A model was constructed of powdered carbonate of iron, about 0.3 of an inch long and 0.1 in thickness, and, by attention to compression, it was arranged that the line of elective polarity through the model was perpendicular to its length. Suspending a thread of cocoon silk with a weight at one end a vertical axis was obtained; a bit of card was then slit and fitted on to the axis, so that when the model was laid on one side, the card stood like a little horizontal table in the middle of the magnetic field. The length of the model extended from the central axis to the edge of the card, so that when the mass swung round, its line of elective polarity was tangent to the circle described.

When the model was made to stand between the flat-faced poles obliquely, the moment the magnet was excited it moved, tending to set its length equatorial and its line of elective

polarity parallel to the lines of magnetic force. In this experiment therefore the model of carbonate of iron, though a magnetic body and strongly attracted by such a magnet as that used, actually *receded* from the magnet.

If, instead of the model of carbonate of iron, we substitute a crystal of sulphate of iron, we have Mr. Faraday's experiment instituted to prove the absence of attraction or repulsion. The dimensions are his dimensions, the arrangement is his arrangement, and the deportment is the exact deportment which he has observed. We have copied his very words, these words being perfectly descriptive of the action of the model. If, then, the experiment be "a striking proof that the effect is not due to attraction or repulsion" in the one case, it must also be such in the other case; but the able originator will, we imagine, hardly push his principles so far. He will, we doubt not, be ready to admit, that it is more probable that a line of elective polarity exists in the crystal, than that a magnecrystallic axis exists in the model*.

By a similar proceeding, using bismuth powder instead of carbonate of iron, the action of Mr. Faraday's plate of bismuth may be exactly imitated. *The objection to the conclusion, that the approach of the magnecrystallic axis, in bismuth, to the magnetic resultant, is due to the repulsion of the planes of cleavage, is thus, we conceive, fairly met.*

Let us look a little further into the nature of this magnecrystallic force, which, as is stated, is neither attraction nor repulsion, but gives position only. The magnecrystallic axis, says Mr. Faraday, *tends* to place itself parallel to the magnetic resultant passing through the crystal; and in the case of a bismuth plate, the recession from the pole and the taking up of the equatorial position is not due to repulsion, but to the *endeavour* the bismuth makes to establish the parallelism before-mentioned. Leaving attraction and repulsion out of the question, we find it extremely difficult to affix a consistent meaning to the words 'tends' and 'endeavour.' "The force is due," says Mr. Faraday, "to that power of the particles which makes them cohere in regular order, and gives the mass its crystalline aggregation, which we call at times the attraction of aggregation, and so often speak of as acting at insensible distances." We are not sure that we fully grasp the meaning of the philosopher in the present instance; for the difficulty of supposing that what is here called the attraction of aggregation, considered apart from magnetic attraction or repulsion,

* The term magnecrystallic axis may with propriety be retained, even should our views prove correct; but then it must be regarded as a subdenomination of the line of elective polarity.

can possibly cause the rotation of the *entire mass* round an axis, and the taking up of a fixed position by the mass, with regard to surrounding objects, appears to us insurmountable. We have endeavoured to illustrate the matter, to our own minds, by the action of a piece of leather brought near a red-hot coal. The leather will be affected and motion caused, without the intervention of either attraction or repulsion, in the present sense of these terms; but this motion exhibits itself in an *alteration of shape*, which is not at all the case with the crystal. Even if the direct attraction or repulsion of the poles be rejected, we do not see how the expressed relation between the magnecrystallic axis and the magnetic resultant is possible, without including the idea of *lateral attraction* between these lines, and consequently of the mass associated with the former. In the case of flat poles, the magnetic resultant is a straight line from pole to pole across the magnetic field. Let us suppose, at any given moment, this line and the magnecrystallic axis of a properly suspended crystal to cross each other at an oblique angle; let the crystal be forgotten for a moment, and the attention fixed on those two lines. Let us suppose the former line fixed, and the latter free to rotate, the point of intersection being regarded as a kind of pivot round which it can turn. On the evolution of the magnetic force, the magnecrystallic axis *will* turn and set itself alongside the magnetic resultant. The matter may be rendered very clear by taking a pair of scissors, partly open, in the hand, holding one side fast, and then closing them. The two lines close in a manner exactly similar; and all that is required to make the illustration perfect, is to suppose this power of closing suddenly developed *in the scissors themselves*. How should we name a power resident in the scissors and capable of thus drawing the blades together? It may be called a 'tendency,' or an 'endeavour,' but the word *attraction* seems to be as suitable as either.

The symmetry of crystalline arrangement is annihilated by reducing the mass to powder. "That force among the particles which makes them cohere in regular order" is here ineffective. The magnecrystallic force, in short, is reduced to nothing, but we have the same results. If, then, the principle of elective polarity, the mere modification of magnetism or diamagnetism by mechanical arrangement, be sufficient to explain the entire series of crystalline phenomena in the magnetic field, why assume the existence of this new force, the very conception of which is attended with so many difficulties*?

* "Perhaps," says Mr. Faraday, in a short note referring to 'the strange and striking character' of this force, "these points may find their explanation hereafter in the action of contiguous particles."

Application of the principle of Elective Polarity to Crystals.

We shall now endeavour to apply the general principle of elective polarity to the case of crystals. This principle may be briefly enunciated as follows:—

If the arrangement of the component particles of any body be such as to present different degrees of proximity in different directions, then the line of closest proximity, other circumstances being equal, will be that chosen by the respective forces for the exhibition of their greatest energy. If the mass be magnetic, this line will stand axial; if diamagnetic, equatorial.

From this point of view, the deportment of the two classes of crystals, represented by iceland spar and carbonate of iron, presents no difficulty. Their crystalline form is the same; and as to the arrangement of the particles, what is true of one will be true of the other. Supposing, then, the line of closest proximity to coincide with the optic axis; this line, according to the principle expressed, will stand axial or equatorial, as the mass is magnetic or diamagnetic, which is precisely what the experiments with these crystals exhibit.

Analogy, as we have seen, justifies the assumption here made. It will, however, be of interest to inquire, whether any discoverable circumstance connected with crystalline formation exists, upon which the difference of proximity depends; and, knowing which, we can pronounce with tolerable certainty, as to the position which the crystal will take up in the magnetic field.

The following experiments will perhaps suggest a reply.

If a prism of sulphate of magnesia be suspended between the poles with its axis horizontal, on exciting the magnet the axis will take up the equatorial position. This is not entirely due to the form of the crystal; for even when its axial dimension is shortest, the axis will assert the equatorial position; thus behaving like a magnetic body, setting its longest dimension from pole to pole.

Suspended from its end with its axis vertical, the prism will take up a determinate oblique position. When the crystal has come to rest, let that line through the mass which stands exactly equatorial be carefully marked. Lay a knife-edge along this line, and press it in the direction of the axis. The crystal will split before the pressure, disclosing a shining surface of cleavage. This is the only cleavage the crystal possesses, and it stands equatorial.

Sulphate of zinc is of the same form as sulphate of magnesia, and its cleavage is discoverable by a process exactly similar

to that just described. Both crystals set their planes of cleavage equatorial. Both are diamagnetic.

Let us now examine a magnetic crystal of similar form. Sulphate of nickel is, perhaps, as good an example as we can choose. Suspended in the magnetic field with its axis horizontal, on exciting the magnet the axis will set itself from pole to pole; and this position will be persisted in, even when the axial dimension is shortest. Suspended from its end, the crystalline prism will take up an oblique position with considerable energy. When the crystal thus suspended has come to rest, mark the line along its end which stands *axial*. Let a knife-edge be laid on this line, and pressed in a direction parallel to the axis of the prism. The crystal will yield before the edge, and discover a perfectly clean plane of cleavage.

These facts are suggestive. The crystals here experimented with are of the same outward form; each has but one cleavage; and the position of this cleavage, with regard to the form of the crystal, is the same in all. The magnetic force, however, at once discovers a difference of action. *The cleavages of the diamagnetic specimens stand equatorial; of the magnetic, axial.*

A cube cut from a prism of scapolite, the axis of the prism being perpendicular to two of the parallel faces of the cube, suspended in the magnetic field, sets itself with the axis of the prism from pole to pole.

A cube of beryl, of the same dimensions, with the axis of the prism from which it was taken also perpendicular to two of the faces, suspended as in the former case, sets itself with the axis equatorial. Both these crystals are magnetic.

The former experiments showed a dissimilarity of action between magnetic and diamagnetic crystals. In the present case both are magnetic, but still there is a difference; the axis of the one prism stands axial, the axis of the other equatorial. With regard to the explanation of this, the following fact is significant. Scapolite cleaves *parallel* to its axis, while beryl cleaves *perpendicular* to its axis; the cleavages in both cases, therefore, stand axial, thus agreeing with sulphate of nickel. The cleavages hence appear to take up a determinate position, regardless of outward form, and seem to exercise a ruling power over the deportment of the crystal.

A cube of saltpetre, suspended with the crystallographic axis horizontal, sets itself between the poles with this axis equatorial.

A cube of topaz, suspended with the crystallographic axis horizontal, sets itself with this axis from pole to pole.

We have here a kind of complementary case to the former. Both these crystals are diamagnetic. Saltpetre cleaves parallel

to its axis; topaz perpendicular to its axis. The planes of cleavage, therefore, stand in both cases equatorial, thus agreeing with sulphate of zinc and sulphate of magnesia*.

Where do these facts point? A moment's speculation will perhaps be allowed us here. May we not suppose these crystals to be composed of layers indefinitely thin, laid side by side, within the range of cohesion, which holds them together, but yet not in absolute contact? This seems to be no strained idea; for expansion and contraction by heat and cold compel us to assume that the particles of matter in general do not touch each other; that there are unfilled spaces between them. In such crystals as we have described, these spaces may be considered as alternating with the plates which compose the crystal. From this point of view it seems very natural that the magnetic laminæ should set themselves axial, and the diamagnetic equatorial; for in crossing transverse to the cleavage, the respective forces would encounter the obstacle presented by the intervening spaces; while in the direction parallel to the cleavages no such obstacle exists†.

We have a very fine description of sand-paper here. The sand or emery on the surface is magnetic, while the paper itself is comparatively indifferent. By cutting a number of stripes of this paper, an inch long and a quarter of an inch wide, and gumming them together so as to form a parallelopiped, we have a model of magnetic crystals which cleave parallel to their axes; the layer of sand representing the magnetic crystalline plate, and the paper the intermediate space between two plates. For such a model one position only is possible between the poles, the axial. If, however, the parallelopiped be built up of squares, equal in area to the cross section of the model just described, by laying square upon square until the pile reaches the height of an inch, we have a model of those magnetic crystals which cleave perpendicular to their axes. Such a model, although its length is four times its thickness, and the whole strongly magnetic, will, on closing the circuit, recede from the poles as if repelled, and take up

* Topaz possesses other cleavages, but for the sake of simplicity we have not introduced them; more especially as they do not appear to vitiate the action of the one introduced, which is by far the most complete.

† In these speculations we have made use of the commonly received notion of matter. Mr. Faraday, for reasons derived from electric conductivity, and from certain anomalies with regard to the combinations of potassium and other bodies, considers this notion erroneous. Nothing, however, could be easier than to translate the above into a language agreeing with the views of Mr. Faraday. The intervals of space between the laminæ would then become intervals of *weaker force*, and the result of our reasoning would be the same as before.

the equatorial position with great energy. The deportment of the first model is that of scapolite; of the second, that of beryl. By using a thin layer of bismuth paste instead of the magnetic sand, the deportment of saltpetre and topaz will be accurately imitated.

Our fundamental idea is, that crystals of one cleavage are made up of plates indefinitely thin, separated by spaces indefinitely narrow. If, however, we suppose two cleavages existing at right angles to each other, then we must relinquish the notion of plates and substitute that of little parallel bars; for the plates are divided into such by the second cleavage. If we further suppose these bars to be intersected by a cleavage at right angles to their length, then the component crystals will be little cubes, as in the case of rock-salt and others. By thus increasing the cleavages, the original plates may be subdivided indefinitely, the shape of the little component crystal bearing special relation to the position of the planes*. It is an inference which follows immediately from our way of viewing the subject, that if the crystal have several planes of cleavage, but all parallel to the same straight line, this line, in the case of magnetic crystals, will stand axial; in the case of diamagnetic, equatorial. It also follows, that in the so-called regular crystals, in rock-salt, for instance, the cleavages annul each other, and consequently no directive power will be exhibited, which is actually the case. Everything which tends to destroy the cleavages tends also to destroy the directive power; and here the temperature experiments of Mr. Faraday receive at once their solution. Crystals of bismuth and antimony lose their directive power *just as they melt*, for at this particular instant the cleavages disappear. Iceland spar and tourmaline, on the contrary, retain their directive power, for in their case the cleavages are unaffected. The deportment of rock crystal, whose weakness of action appears to have taken both Mr. Faraday and M. Plücker by surprise—as here the optic axis force, without assigning any reason, has thought proper to absent itself almost totally—follows at once from the homogeneous nature of its mass; it is almost like glass, which possesses no directive power; its cleavages are merely traces of cleavage. If, instead of possessing planes of cleavage, a crystal be composed of a bundle of fibres, the forces may be expected to act with greater energy along the fibre than across it. Anything, in short, that affects the mechanical arrangement of the particles will affect, in a corresponding degree, the line of elective polarity. There are crystals which are both fibrous and have planes of cleavage, the latter often per-

* See the last note at bottom of page 23.

pendicular to the fibre; in this case two opposing arrangements are present, and it is difficult to pronounce beforehand which would predominate*.

The same difficulty extends to crystals possessing several planes of cleavage, oblique to each other, and having no common direction. In many cases, however, the principle may be successfully applied. We shall content ourself in making use of it to explain the deportment of that class of crystals, of which, as to form, Iceland spar is the type.

For the sake of simplicity, we will commence our demonstration with an exceedingly thin rhombus cloven from this crystal. Looking down upon the flat surface of such a rhombus, what have we before us? It is cleavable parallel to the four sides. Hence our answer must be, "an indefinite number of smaller rhombuses held symmetrically together by the force of cohesion." Let us confine our attention, for a moment, to two rows of these rhombuses; the one ranged along the greater diagonal, the other along the less. A moment's consideration will suffice to show, that whatever be the number of small rhombuses supposed to stand upon the long diagonal, precisely the same number must fit along the short one; but in the latter case *they are closer together*. The matter may be rendered very plain by drawing a lozenge on paper, with opposite acute angles of 77° , being those of Iceland spar. Draw two lines, a little apart, parallel to two opposite sides of the lozenge, and nearly through its centre; and two others, the same distance apart, parallel to the other two sides of the figure. The original rhombus is thus divided into four smaller ones; two of which stand upon the long diagonal, and two upon the short one, each of the four being separated from its neighbour by an interval which may be considered to represent the interval of cleavage in the crystal. The two which stand upon the long diagonal, L, have their acute angles opposite; the two which stand upon the short diagonal, S, have their obtuse angles opposite. The distance between the two former, across the interval of cleavage, is to the distance between the two latter, as L is to S, or as the cosine of $38^\circ 30'$ to its sine, or as 4 : 3. We may conceive the size of these rhombuses to decrease till they become molecular; the above ratio will then appear in the form of a differential quotient, but its value will be unaltered. Here, then, we have along the greater diagonal a row of magnetic or diamagnetic molecules, or 'centres of force,' to use a term of Mr. Faraday's, the distance between each two being represented by the figure

* It is probable that the primitive plates themselves have different arrangements of the molecules *along* and *across* them.

4; and along the short diagonal a row of molecules, the distance between each two being represented by the figure 3. In the magnetic field, therefore, the short diagonal will be the line of elective polarity; and in magnetic crystals will stand axial, in diamagnetic equatorial, which is precisely the case exhibited by experiment. Thus the apparent anomaly of carbonate of lime setting its long diagonal axial, and carbonate of iron its short diagonal axial, seems to be fully explained; the position of the former line being due, not to any endeavour on its part to stand parallel with the magnetic resultant, but being the simple consequence of the repulsion of the short diagonal.

These conclusions are corroborated by experiment. Let a rhombus be cut from pasteboard, of the same shape as a rhombus of Iceland spar. Along the greater diagonal, fix six or eight small magnetic pellets, and along the short diagonal the same number. If this rhombus be suspended horizontally in the magnetic field, on closing the circuit the short diagonal will set itself from pole to pole. If pellets of bismuth be used, the same diagonal will stand equatorial.

There is no difficulty in extending the reasoning used above to the case of full crystals. If this be done, it will be seen that the line of closest proximity coincides with the optic axis, which axis, in the magnetic field, will signalize itself accordingly. A remarkable coincidence exists between this view and that expressed by Mitscherlich in his beautiful investigations on the expansion of crystals by heat*. "If," says this gifted philosopher, "we imagine the repulsive force of the particles increased by the accession of heat, then we must conclude that the line of greatest expansion will be that in which the atoms lie most closely together." This line of greatest expansion Mitscherlich found, in the case of Iceland spar, to coincide with the optic axis. The same conclusion has thus been arrived at by two modes of reasoning, as different as can well be conceived.

If, then, speculation and experiment concur in pronouncing the line of closest proximity among the particles, to be that in which the magnetic and diamagnetic forces will exhibit themselves with peculiar energy, thus determining the position of the crystalline mass between the poles, we are furnished with a valuable means of ascertaining the relative values of this proximity in different directions through the mass. An *order of contact* might, perhaps, by this means be established, of great interest in a mineralogical point of view. In the case of a right rhombic prism, for example, the long diagonal of the base

* Poggendorff's *Annalen*, vol. x. p. 138.

may denote a line of contact very different from that denoted by the short one; and the line at right angles to the diagonals, that is, the axis of the prism, a contact very different from both. We can compare these lines two at a time. By hanging the short diagonal vertical in the magnetic field, its rotatory power is annulled, and we can compare the long diagonal and the axis. By hanging the long diagonal vertical, we can compare the short diagonal and the axis. By hanging the axis vertical, we can compare the two diagonals. From this point of view the deportment of heavy spar and cœlestine, so utterly irreconcilable with the assumption of an optic axis force, presents no difficulty. If we suppose the proximity along the axis of the prism to be *intermediate* between the proximities along the two diagonals, the action of both crystals follows as a necessary consequence. Suspended from one angle, the axis must stand from pole to pole; from the other angle, it must stand equatorial.

A ball of dough, made from bismuth powder, was placed between two bits of glass and pressed to the thickness of a quarter of an inch. It was then set edgeways between the plates and pressed again, but not so strongly as in the former case. A model of heavy spar was cut from the mass, so that the shorter diagonal of its rhombic base coincided with the line of greatest compression; the axis of the model with the direction of less compression, and the longer diagonal of the base with that direction in which no pressure had been exerted. *When this model was dried and suspended in the magnetic field, there was no recognizable difference between its deportment and that of heavy spar.*

When a crystal cleaves symmetrically in several planes, all parallel to the same straight line, and, at the same time, in a direction perpendicular to this line, then the latter cleavage, if it be more eminent than the former, may be expected to predominate; but when the cleavages are oblique to each other, the united action of several minor cleavages may be such as to overcome the principal one, or so to modify it that its action is not at all the same as that of a cleavage of the same value unintersected by others. A complex action among the particles of the crystal itself may contribute to this result, and possibly in some cases modify even the influence of proximity. If we hang a magnetic body between the poles, it always shows a preference for edges and corners, and will spring to a point much more readily than to a surface. Diamagnetic bodies will recede from edges and corners. The fluid is, as it were, discharged with greater power from a point. A similar action among the crystalline particles may possibly bring about the modification we have hinted at.

During this investigation a great number of crystals have passed through our hands, but it is useless to cumber the reader with a recital of them. The number of natural crystals have amounted to nearly one hundred; while, through the accustomed kindness of Professor Bunsen, the entire collection of artificial crystals, which his laboratory contains, has been placed at our disposal*.

We now pass over to a brief examination of the basis on which the second law of M. Plücker rests:—the affirmation that the magnetic attraction decreases in a quicker ratio than the repulsion of the optic axes. The ingenuity of this hypothesis, and its apparent sufficiency to account for the phenomena observed by M. Plücker, are evident. It will be seen, however, that this repulsion arises from quite another cause—a source of error which, unfortunately, has run undetected through the entire series of this philosopher's inquiries.

The following experiment is a type of those which led M. Plücker to the above conclusion. A tourmaline crystal 36 millimeters long and 4 millimeters across was brought between a pair of pointed moveable poles, so that it could barely swing between them. It set itself *axial*. On removing the poles to a distance and again exciting the magnet the crystal stood *equatorial*. The same occurred, if the poles were allowed to remain as in the former case, when the crystal was raised above them or sunk beneath them. *According as the crystal was withdrawn from the immediate neighbourhood of the poles, it turned gradually round and finally set itself equatorial†.*

A similar action was observed with staurolite, beryl, idocrase, smaragd, and other crystals.

We have repeated these experiments in the manner described, and obtained the same results. A prism of tourmaline three-quarters of an inch long and a quarter of an inch across was hung between a pair of poles with conical points, placed an inch apart. On exciting the magnet the crystal stood axial. When the poles were withdrawn to a distance and the force again evolved, the same crystal stood equatorial. An exceedingly weak current was here used; a single cell of Bunsen's construction being found more than sufficient to produce the result.

According to the theory under consideration, the tourmaline, in the first instance, stood from pole to pole because the magnetism was strong enough to overcome the repulsion of the optic axis. This repulsion, decreasing more slowly than

* We gladly make use of this opportunity to express our obligation to Dr. Debus, the able assistant in the chemical laboratory.

† Poggendorff's *Annalen*, vol. lxxii. p. 319.

the magnetic attraction, necessarily triumphed when the poles were removed to a sufficient distance. The same crystal, however, between a pair of flat poles, could *never* take up the axial position. On bringing the faces within half an inch of each other, and exciting the magnet by a battery of thirty-two cells, the crystal vibrated between the faces without touching either. The same occurred when one cell, six cells, twelve cells, and twenty cells, respectively, were employed.

If the attraction increases, as stated, more quickly than the hypothetic repulsion, how can the impotence of attraction in the case before us be accounted for? We have here a powerful current, and poles only half an inch apart; power and proximity work together, but their united influence is insufficient to pull the crystal into the axial line. The cause of the phænomena must it seems be sought, not in optic repulsion, but in the manner in which the magnetic force is applied. The crystal is strongly magnetic, and the pointed poles exercise a concentrated *local* action. The *mass* of both ends of the crystal, when in the neighbourhood of the points, is powerfully attracted, while the action on the central parts, on account of their greater distance, is comparatively weak. Between the flat poles the crystal finds itself, as it were, totally immersed in the magnetic influence; its entire mass is equally affected, and the whole of its directive power developed. The similarity of action between the flat poles and the points, *withdrawn to a distance*, is evident. In the latter case, the force, radiating from the points, has time to diffuse itself, and fastens almost uniformly upon the entire mass of the crystal, thus calling forth, as in the former case, its directive energy; and the equatorial position is the consequence. The disposition of the lines of force, in the case of points, is readily observed by means of iron filings strewed on paper and brought over the poles. When the latter are near each other, on exciting the magnet the filings are gathered in and stretch in a rigid line from point to point; according as the poles are withdrawn, the magnetic curves take a wider range, and at length attain a breadth sufficient to encompass the entire mass of the crystal*.

As the *local attraction* of the mass in the case of magnetic crystals deranges the directive power and overcomes it, so will the *local repulsion* of the mass in diamagnetic crystals. A prism of heavy spar, whose length was twice its breadth, hung from its acute angle, stood between the flat poles axial, between the points equatorial. On making its length and

* Mr. Faraday has already pointed out "the great value of a magnetic field of uniform force."—Phil. Trans., 1849, p. 4.

breadth alike, the axis of the prism stood from pole to pole, whether the conical points or flat faces were used. Shortening the axial direction a little more, and suspending the crystal from its obtuse angle, the axis between the flat poles stood equatorial, and, consequently, the longest dimension of the crystal, axial; between the points, owing to the repulsion of the extreme ends, the length stood equatorial. Similar experiments were made with coëlestine and topaz; but all with the same general result.

"I had the advantage," says Mr. Faraday, "of verifying Plücker's results under his own personal tuition, in respect of tourmaline, staurolite, red ferrocyanide of potassium, and Iceland spar. Since then, and in reference to the present inquiry, I have carefully examined calcareous spar, as being that one of the bodies which was at the same time free from magnetic action, and so simple in its crystalline relations as to possess but one optic axis.

"When a small rhomboid about 0.3 of an inch in its greatest dimension is suspended with its optic axis horizontal between the pointed poles of the electro-magnet, approximated as closely as they can be to allow free motion; the rhomboid sets in the equatorial direction, and the optic axis coincides with the magnetic axis; but if the poles be separated to the distance of a half or three-quarters of an inch, the rhomboid turned through 90° and set with the optic axis in the equatorial direction, and the greatest length axial. In the first instance the diamagnetic force overcame the optic axis force; in the second the optic axis force was the stronger of the two."

The foregoing considerations will, we believe, render it very clear that the introduction of this optic axis force is altogether unnecessary; the case being simply one of local repulsion. Mr. Faraday himself found that the crystal between the flat poles could *never* set its optic axis from pole to pole; between the points alone was the turning round of the crystal possible. We have made the experiment. A fine large crystal of Iceland spar, suspended between the near points, set its optic axis from point to point; between the distant points the axis stood equatorial. The crystal was then removed from the magnetic field, placed in an agate mortar and pounded to powder. The powder was dissolved in muriatic acid. From the solution it was precipitated by carbonate of ammonia. The precipitate thus obtained, as is well known, is exactly of the same chemical constitution as the crystal. This precipitate was mixed with gum water and squeezed in one direction. From the mass thus squeezed a model of Iceland spar was made, the line of greatest compression through

the model coinciding with that which represented the optical axis. *This model imitated, in every respect, the deportment observed by Mr. Faraday.* Between the near points the optical axis stood from point to point, between the distant points equatorial. It cannot however be imagined that the optic axis force survived the pounding, dissolving and precipitating. Further, this optic axis force is a sword which cuts two ways; if it be assumed repulsive, then the deportment of carbonate of lime and iron is unexplainable; if attractive, it fails in the case of Iceland spar.

It is a remarkable fact, that all those crystals which exhibit this phenomenon of turning round, cleave, either perpendicular to their axes, or oblique to them, furnishing a resultant which acts in the direction of the perpendicular. Beryl is an example of the former; the crystal just examined, Iceland spar, is an example of the latter. This is exactly what must have been expected. In the case of a magnetic crystal, cleavable parallel to its length alone, there is no reason present why the axial line should ever be forsaken. But if the cleavages be transverse, or oblique, so as to furnish a line of elective polarity in the transverse direction, two diverse causes come into operation. By virtue of its magnetism, the crystal seeks to set its length axial, as a bit of iron or nickel would do; but in virtue of its *mechanical structure*, it seeks to place a line at right angles to its length axial. For the reasons before adduced, if the *near* points be used, the former is triumphant; if the points be distant, the latter predominates.

We noticed in a former paper a description of gutta-percha of a fibrous texture, which, on being suspended between the poles, was found to transmit the magnetic force with peculiar facility along the fibre. A piece was cut from this substance, exactly the same size as the tourmaline crystal, described at the commencement of this section. The fibre was transverse to the length of the piece. Suspended in the magnetic field, the gutta-percha exhibited all the phænomena of the crystal.

One of the sand-paper models before described is still more characteristic as regards this turning round on the removal of the poles to a distance. We allude to that whose magnetic layers of emery are perpendicular to its length. The deportment of this model, if we except its greater energy, is not to be distinguished from that of a prism of beryl. Between the near points both model and crystal stand axial, between the distant points equatorial, and between the flat poles the deportment, as before described, is exactly the same. The magnetic laminæ of beryl occupy the same position, with regard to its axis, as the magnetic laminæ of the model, with

regard to its axis. There is no difference in construction, save in the superior workmanship of nature, and there is no difference at all as regards deportment. Surely these considerations suggest a common origin for the phænomena exhibited by both.

We have the same action in the case of the compressed dough, formed from the powdered carbonate of iron and bismuth. A plate of the former, three-quarters of an inch square and one-tenth of an inch in thickness, stands between the conical poles, brought within an inch of each other, exactly axial; between the same poles, two inches apart, it stands equatorial. A plate of compressed bismuth dough stands, between the near points, equatorial, between the distant points, axial.

Any hypothesis which solves these experiments must embrace crystalline action also; for the results are not to be distinguished from each other. But in the above cases an optic action is out of the question. With the similarity of structure between beryl and the sand-paper model, above described,—with the complete identity of action which they exhibit, before us; is it necessary, in explanation of that action, to assume the existence of a force which, in the case of the crystal, is all but inconceivable, and in the case of the model is not to be thought of? In his able strictures on the theory of M. Becquerel*, M. Plücker himself affirms, that we have no example of a force which is not associated with ponderable matter. If this be the case as regards the optic axis force, if the attraction and repulsion attributed to it be actually exerted on the mass of the crystal, *how is it to be distinguished from magnetism or diamagnetism?* The assumption of Mr. Faraday appears to be the only refuge here; the abandonment of attraction and repulsion altogether.

In the first section of this memoir it has been proved, by the production of numerous exceptions, that the law of M. Plücker, as newly revised, is untenable. It has also there been shown, that the experiments upon which Mr. Faraday grounds his hypothesis of a purely directive force, are referable to quite another cause. In the second section an attempt has been made to connect this cause with crystalline structure, and to prove its sufficiency to produce the particular phænomena exhibited by crystals. In the third section we find the principle entering into the most complicated instances of these phænomena, and reducing them to cases of extreme simplicity. The choice therefore rests between the assumption of *three new forces* which seem but lamely to execute their mis-

* Poggendorff's *Annalen*, vol. lxxvii. p. 578.

sion, and that simple modification of existing forces, to which we have given the name elective polarity, and which seems sufficiently embracing to account for all.

It appears then to be sufficiently established, that from the deportment of crystalline bodies in the magnetic field, no direct connexion between light and magnetism can be inferred. A rich possession, as regards physical discovery, seems to be thus snatched away from us; but the result will be compensatory. That a certain relation exists, with respect to the path chosen by both forces through transparent bodies, must be evident to any one who carefully considers the experiments described in this memoir. The further examination of this deeply interesting subject we refer to another occasion.

Nature acts by general laws, to which the terms great and small are unknown; and it cannot be doubted that the modifications of magnetic force, exhibited by bits of copperas and sugar in the magnetic field, display themselves on a large scale in the crust of the earth itself. A lump of stratified grit exhibits elective polarity. It is magnetic, but will set its planes of stratification from pole to pole, though it should be twice as long in the direction at right angles to these planes. A new element appears thus to enter our speculations as to the position of the magnetic poles of our planet; the influence of stratification and plutonic disturbance upon the magnetic and electric forces.

Marburg, May 1850.

II. *Notices of a late Visit to the Parallel Roads of Lochaber.*

By JAMES BRYCE, JUN., M.A., F.G.S.*

THE Lochaber glens have been subjected to so keen a scrutiny by the advocates for the various theories of the Parallel Roads, that it cannot be expected there should remain many facts of importance to be yet ascertained. By this circumstance, however, the obligation upon an observer at once to make known such facts as may have come under his notice is rendered more imperative, while the value of new facts is enhanced. Observations, which in other circumstances would be scarcely deemed worthy of record, become of importance when viewed in connexion with an inquiry such as this, which, after all the discussion elicited by it, still remains the great unsolved problem of Scottish geology. In submitting the following communication, it is not my purpose to advance a new

* Communicated by the Author, and containing the substance of a paper read to the Philosophical Society of Glasgow, March 6, 1850.

theory. I have merely in view the much more humble object of putting on record a few facts, which seem to have escaped the notice of previous observers; and of offering, in connexion with these, some remarks on the two theories last proposed. I refer to those of Mr. Chambers of Edinburgh, and Mr. James Thomson of Glasgow, both published early in 1848; the latter immediately before my visit, which took place in July of that year. My examination of the district had thus additional interest given to it, as the facts were to be viewed under a somewhat novel aspect, and had not yet been commented on by any geologist, with reference to their bearing upon the two theories in question.

Mr. Chambers's account of the Parallel Roads, with his theory of their origin, forms a portion (pp. 95–130) of his valuable and beautifully illustrated work on Ancient Sea-Margins. A map of part of Lochaber, showing the shelves in the glens, is given at the end. It has been “constructed by Messrs. W. and A. K. Johnston under the direction of Sir George McKenzie, Bart., David Milne, Esq., and Robert Chambers, Esq.” The same map accompanies a late paper on the Parallel Roads, by Sir George McKenzie (Ed. N. Phil. Journ., vol. xlv.) ; it is that to which Mr. Milne refers in his late important paper (Ed. N. Phil. Journ., vol. xliii. p. 339); and on which the reasonings of Mr. James Thomson are founded, an enlarged copy of it having been laid before the Royal Society of Edinburgh along with his paper.

Now, this map contains an important topographical error, calculated to mislead those who may frame theories of the Roads without having made a personal inspection of the ground. The error consists in this—that at its junction with Glen Fintec, Glen Gluoy is laid down as opening towards Loch Lochy; whereas, in point of fact, the high ridge descending from the table-land at the top of Glen Toorat, and shutting in Glen Gluoy on the west, continues its course southwards fully a mile below the point where Glen Fintec opens into Glen Gluoy. Glen Fintec is *thus completely cut off from direct connexion with Loch Lochy*, the ridge in question being continuous throughout, and rising to the height of from 1200 to 1800 feet above the sea, or from 300 to 700 feet above the upper shelf. The rocks of which the ridge consists are chiefly micaceous slate and quartzite, the strata being nearly on end, and ranging in the direction of the ridge, or about S.W. I could detect no traces of scratching or grooving, though the rocks are laid bare in many places, and strew the surface in huge flat masses.

The error now pointed out involves another in the repre-

sentation of a portion of the upper shelf. The eastern portion is correctly represented as terminating at the south-west corner of Glen Fintec; but on the west side, the shelf, instead of terminating as expressed on the map, is continued a considerable distance southwards of the opening of Glen Fintec, from half a mile to a mile, or perhaps more; at first less distinct than usual, then more plainly marked, till coming against a rocky projecting ledge on the hill side, it fails as usual to impress it, and is seen no more.

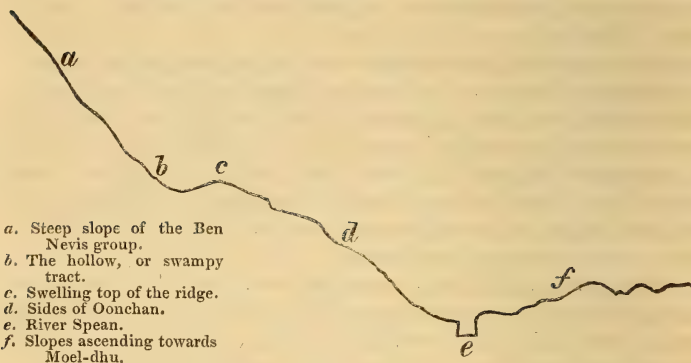
On referring lately to Sir T. D. Lauder's map accompanying his paper (Trans. Roy. Soc. Edinb. for 1817), which I had not looked into before visiting the Parallel Roads, I found that his representation of this portion of the district is much more correct. Glen Gluoy is given in its true dimensions; and the stream formed by the union of the Gluoy and Fintec waters is laid down as turning, at a place called Lowbridge, round the southern termination of the mountainous ridge just described, and discharging into Loch Lochy, nearly opposite to a village named Kyle-Rose in Mr. Chambers's map. This representation is very near the truth; but perhaps too great extension is given to the southern part of Loch Lochy.

One of the principal objections which has been urged against Mr. Milne's theory, is the absence from the district of a sufficient quantity of detrital matter to account for the barriers at the mouths of the glens, required by the theory. The force of this objection would be very much diminished, if we could receive Mr. Chambers's account of the hill of Oonchan as correct. It appears to me, however, that he quite over-estimates the amount of detritus in this hill.

After giving a full and accurate description of the other principal detrital accumulations of the district, Mr. Chambers thus notices the hill of Oonchan:—"By far the grandest delta of the district is that hill which has been referred to under the name of Unichan as occupying so much of the lower part of Glen Spean. This is a mass of gravel 11 miles long by perhaps 2 broad, and reaching an elevation of 612 feet. I observed rock rising through it at one place; but it is mainly, as has been said, a hill of gravel." He considers that "when the sea stood somewhat above 622 feet (and there is evidence of its having paused long at 628 or 630), the rivers descending from the Ben Nevis group of mountains delivered their spoils into the estuary filling Glen Spean: on the withdrawal of the sea this mass was left."

The high ground in question, part only of which is called Oonchan, is an undulating ridge parallel to the main chain,

and stretching from near Fort William to within $1\frac{1}{2}$ mile of the bridge of Roy, a distance of about 12 miles. Such subordinate elevations are seen at the base of almost every high chain, and mark the axes along which the upheaving forces acted with decreasing intensity. This ridge is separated from the main chain by a slightly depressed tract, having a very smooth outline, into which five glens descending from the Ben Nevis group open at right angles, the surface presenting no marked change of character at the junction. The streams from these glens, as well as those which drain the tract itself, being prevented by the high ground in front from following direct courses to the valley of the Spean, are deflected to the east and west, parallel to the high ground on either side. The watershed of the tract being nearer the western than the eastern end, and the inclination eastwards slight, there is an imperfect discharge of the waters, and consequently extensive swamps have been formed, which sometimes become lakes. The annexed sketch will give an idea of the outline of the surface.



On the western part of the ridge the rock is seen in many places; and about the middle I found it a little lower than the highest point, *c*, of the ridge at that part; and I think there can be little doubt that the thickness of the detrital covering is in most places inconsiderable. At its eastern termination detritus appears in more imposing quantity. Near the bridge of Roy the end of the ridge is cut through by numerous streams, or rather the channels of streams; for there is often no water, and the detritus stands out in numerous round or elliptic flat-topped mounds with steep sides. Towards the base of Cruachaninish and Benchilinaig these are smaller, and of rounder forms, resembling Danish raths;

while further back the detritus only shows itself in terraces, formed by the streams cutting into the talus at the base of the high mountains.

Mr. Chambers regards the question of the origin of the Parallel Roads as "involved in that of the superficial formations generally, which bear the marks of former levels of the sea at various intervals up to 1200 feet;" the various markings in the three kingdoms, in France, &c., "all falling into such conformity as to prove that the shift of level has been effected from at least that height with perfect equability throughout." He considers this widely extended and strongly marked conformity "as more favourable to the idea of a recession of the sea as opposed to that of an elevation of the land, since it is precisely what would result from the former operation, while there is an obvious difficulty in supposing" that so large a portion of the earth's crust could be repeatedly upheaved, and yet the relative levels so preserved "that between Paris and Inverness not a vertical foot of derangement could be detected."

The explanation of the origin of the Parallel Roads is thus mixed up with, indeed forms an essential part of, his general theory. And whatever difficulty geologists may feel in giving their assent to such generalizations as those just quoted, or however unwilling they may be, in the present state of inquiry, to admit many successive equable sinkings of the waters of the ocean all over the globe, the same difficulties and hesitation must be experienced in receiving Mr. Chambers's explanation as the true theory of the Parallel Roads. Besides, the *speciality* of the phenomena is by no means accounted for on this hypothesis. It appears to me to require a special local cause. On the hypothesis of the shelves being formed by the sea, it cannot, I think, be shown why other Highland glens were not equally impressed; or that any conservative influences have operated in Lochaber, which were not just as likely to prevail in other places. This argument cannot be properly estimated by one who has not seen the shelves in Glen Roy and Glen Gluoy; from examining sea and lake-terraces, from descriptions and drawings, the faintest conceptions only can be formed of the wonderful reality. Any one on whose view the scene which is presented on turning the flank of Bohuntine hill, bursts for the first time, must look with the deepest astonishment at the distinctness, continuity, and extent of the shelves; he will feel how inadequate were all his conceptions, and how little the Parallel Roads have in common with any appearances which have come under his notice before. Mr. Chambers eloquently describes the first impressions, and acknow-

ledges the "singular distinctness" of the shelves in this locality; yet his theory affords no explanation of a phænomenon so remarkable. But this argument has been so ably handled by Mr. Milne in his reply to Mr. Darwin (Ed. N. Phil. Journ., vol. xliii. p. 437), that it is unnecessary to insist further upon it.

The faint and higher markings on the south side of Glen Spean, which Mr. Chambers lays so much stress upon as supporting his view, I did not notice. "The whole," he says, "might appear doubtful to many persons; in an unfavourable light, a hasty observer might pass them by altogether unnoticed." These may have been my circumstances, and I do not therefore question the existence of such markings; but I cannot regard the conclusion as warranted by the facts—the existence, namely, "in Glen Spean of a body of water at levels above the barriers assigned to it by McCulloch, Lauder and Milne." Are not these and similar slight and local markings best explained on the received theory,—the action of currents upon the submerged land, or the occasional pauses in the process of elevation?

While thus dissenting from the theoretical conclusions at which Mr. Chambers has arrived, I cannot forbear to express my high admiration of his patient and active research,—his clear, truthful, and eloquent descriptions,—and of the service he has rendered to geology by his many exact measurements, and by proposing a theory which will lead to a more careful study of phænomena of this class.

The lake theory has gained immensely of late by the advocacy of Mr. David Milne. His paper, already referred to, is perhaps the most able which has been written upon the Parallel Roads. The evidence in support of his own views has been collected with the greatest sagacity, and the arguments founded upon it conducted with consummate skill; while he appears to me to have completely demolished both the theory of Mr. Darwin, and the glacial theory, *in the form proposed by M. Agassiz*. The agency assigned by Agassiz will not explain all the phænomena, and is positively inconsistent with many facts. But it does not hence follow that glacial action is to be rejected, as explaining the blocking up of the mouths of the glens,—for it is required for this purpose alone. May not a form be given to the theory which will adapt it to all the exigencies of the case, and thus remove from the lake theory the one great remaining objection—the origin and the disappearance of the enormous earthy barriers at the mouths of the glens? Since Agassiz wrote, the question has been placed on a very different footing. The first glacialist in

Europe, Prof. J. D. Forbes, has given it as his decided opinion that glaciers formerly existed on the Cuchullin hills in Skye (Ed. N. Phil. Journ., vol. xl. p. 79). Why, then, may not masses of ice have filled the still higher valleys of the Ben Nevis group of mountains? Professor Forbes's late discoveries in Switzerland respecting the viscosity of glacier ice, and the nature of glacier motion, appear to have suggested to Mr. James Thomson the highly ingenious modification of the glacial theory lately proposed by him (Ed. N. Phil. Journ., vol. xlv. p. 49). The gist of this theory is contained in the following passage:—

“In Switzerland the mean temperature of the comparatively low and flat land is so much above the freezing-point, that the ice no sooner descends from the mountains than it melts away; and it is thus usually prevented from spreading to any considerable extent over the plains. In the Antarctic continent, on the contrary, the mean temperature is nowhere so high as the freezing-point. The ice, therefore, which descends from the hills unites itself with that which is deposited from the atmosphere on the plains; and the whole becomes consolidated into one continuous mass, of immense depth, which glides gradually onwards towards the ocean Now a climate somewhere intermediate between these extremes appears to be that which would be requisite to form the shelves in the glens of Lochaber. The climate of Switzerland would be too warm to admit of a sufficient horizontal extension of the glaciers; that of the Antarctic continent too cold to allow the lakes to remain unfrozen. If the climate of Scotland were again to become such that the mean temperature of Glen Spean would be not much above the freezing-point, there seems to be every reason to believe that that glen would again be nearly filled with an enormous mass of ice; while its upper parts, and also Glen Roy, would be occupied by lakes”

The state of things here supposed is extremely critical; not likely long to maintain itself under the same geographical distribution of the surface as now prevails, and liable to be changed by many slight causes. If the mean temperature of Glen Spean was little above freezing, and wide fields of ice covered its surface, it is not probable that the lakes in the glens, at considerably higher levels, would long remain unfrozen; and if the Ben Nevis group of mountains, whose mean height we may take at somewhat less than 4000 feet, not only nourished glaciers in their higher recesses, but were wholly enveloped in sheets of ice, can we suppose that the mountains surrounding Glen Roy and Glen Gluoy, many of which attain the altitude of from 2000 to 2500 feet, would not likewise give

origin to masses of ice, descending into the glens, and occupying the very sites of our supposed lakes? On the other hand, it may be stated in favour of Mr. Thomson's views, that the hypothesis of Glen Spean being "filled with an enormous mass of ice" which would block up Glen Roy, is more consistent with the geography of the district, than the supposition that a glacier descended from one of the high valleys of the Ben Nevis group, and forced its way into the opening of Glen Roy. There is nothing in the nature of the country to determine a glacier to follow such a course. The form of the surface between the Lochaber glens and the Ben Nevis group is such, that if a glacier descended from any one of the five great glens, whose directions are inclined to that of Glen Roy at an angle of 60 or 70 degrees, and reached the open country at the base of the mountains, there would be nothing to determine its course up Glen Roy, or indeed in any one direction more than another, except the slight eastward and northward slope already described. Glaciers descending from these glens would thus coalesce into one huge sheet, coextensive with the valley of the Spean. The hypothesis of sheets of ice covering the whole surface—"des grandes nappes de glace"—seems also more consistent with the absence of "perched blocks" and moraines, than the idea of separate glaciers. These are not seen anywhere over the surface of the open tract between the mountains and the river; and the peculiar detrital covering is very like that which would be formed under such advancing sheets, most of it being stratified sand and small gravel, the result of wearing, or decomposition *in situ*.

Mr. Thomson's explanation of the phænomena of Glen Gluoy is very ingenious. It will be remembered that these are peculiar. The shelves do not correspond with those in the other glens; and while in the latter each successive shelf, as we descend, extends further down the glens than those that are higher, in Glen Gluoy the upper shelf extends further towards the mouth of the glen than the lower; and this lower shelf, unlike all the others, is not in connexion with any summit level. If the lake theory be true, it will follow from these facts that the barrier which retained the water at the lower level was further up the glen than that which retained it at the higher; and that when the lower shelf was forming, the overflow must have taken place at the mouth of the glen. Mr. Thomson supposes "that the glacier which occasioned the formation of the higher of the Glen Gluoy shelves had at some former period protruded a terminal moraine as far up the glen as the termination of the lower shelf; that, on the final retiring of the glacier, this old moraine served as a bar-

rier to dam up the water to the level of the lower shelf, and that it has been subsequently washed away by the river flowing over it." He then suggests that the space between the terminations of the upper and lower shelves should be examined, to ascertain if the remains of such a moraine exist. I made this examination with considerable care, but could find no such remnants. There is some detritus in the main glen opposite the mouth of Glen Fintec; but it has obvious reference to the present drainage, and is in no way remarkable. The whole of Glen Gluoy is indeed singularly free from detritus;—a peculiarity which I consider due to its form. It is narrow, and the hills rise steep and high from the very margin of the river, so that there is no space where detritus could rest; and it is thus swept away as soon as it is brought down. This circumstance is also favourable to the rapid and complete removal of such a moraine, or barrier, as Mr. Thomson supposes may have once existed. The mouth of the glen is equally free from detritus, or other indications of the existence of earthy barriers in a former condition of things.

"A glacier occupying the present site of Loch Lochy, and receiving supplies from the neighbouring mountains, would appear," Mr. Thomson says, "to afford a sufficient explanation of the phænomena observed in this glen." This was probably written under the impression that Glen Fintec communicated with Loch Lochy, and that the mouth of Glen Gluoy was in the way of a glacier advancing from that lake. But this is not the case. A glacier having its origin among the high mountains to the N.W. of Loch Lochy—the only hills high enough to produce one—and *advancing from Loch Lochy*, must make its way past Maucomer and Brecklech up the valley of the Spean, for so only will the levels permit. This direction is about perpendicular to that of Glen Gluoy; and it would be only a lateral branch or arm, parting from the main body, that could penetrate that glen. The mouth of the glen is narrow, and the hill sides rise steep and high; a little way up there is a considerable bend before we reach, at a mile's distance, the bosom or *sinus* in the hill side, where the moraine is conceived to have existed in connexion with the lower shelf. All this shows the improbability of a moraine being deposited at this place; and that recourse may as well be had to the masses of ice with which Glen Spean has been supposed to be filled, from its chief source in the Ben Nevis group. But it seems impossible that such masses of ice could deposit a moraine in the situation required; and it even appears doubtful whether sheets of ice would deposit moraines at all.

I do not feel myself competent to express a decided opinion upon this "vexed question;" but regarding the lake theory as the true one, I think it now only remains to be determined whether the barriers at the mouths of the glens consisted of ice or of earthy materials. Perhaps we know nearly as much regarding the latter as we ever can know; but the valley of the Spean has never been carefully examined, with reference to the former passage of glaciers through it, by one fully competent to the task. Till this has been done, geologists are not in a position to decide between the rival theories.

III. *On the great importance of Deviations from the mean state of the Atmosphere for the Science of Meteorology.* By Dr. BUYS BALLOT*.

THERE are some points in meteorology which certainly are no longer disputed, but which nevertheless are not always sufficiently present to the minds of meteorologists, nor so publicly pronounced and admitted as to compel them to action, and serve as a torch to illumine the path leading to the penetralia of science. Though these facts may appear simple, particularly in England, where they have been treated upon, still even there it cannot be considered superfluous to view them in another light, and to introduce them in connexion with a Dutch prize question, which, in my opinion, is the most important meteorological question that can be brought forward, and which fully characterizes the new meteorological period developed by Dove.

On the other hand, England has such boundless merits as to meteorological observations, and has such great facilities, in consequence of her extensive influence and wide-spread observatories, for the application of those facts in all parts of the earth, that she has a right to receive information, through her own periodicals, of what is taking place on the continent. I therefore decided (on being solicited by the Society of Physics of Berlin to send in annually a report of, and opinions on, what might occur in meteorology) to adopt also for insertion in an English periodical, by modifying some points of less interest, a paper drawn up for the Journal of the Society, *Die Fortschritte der Physik*. The truths to which I allude were particularly remarked by me in 1846, when I wrote my work *Les Changements Périodiques de Température dépendants de la nature du Soleil et de la Lune déduits d'observations Néerlandaises de 1729-1846*, in which those parti-

* Communicated by the Astronomer Royal.

culars are to be found, chapter vii. p. 104 *et seq.* But let us proceed to those propositions.

I. The average temperature which prevails at any certain place is not that which is generated there by the action of the sun, &c., and which would depend simply on the latitude and the elevation of the ground, but is remarkably changed by the influences of other regions, particularly by the action of the winds.

II. That average temperature, such as it is obtained anywhere from observations during a series of years, for the different months or days of the year, will by no means always prevail at those places for the determined month or day of each single year. On the contrary, observations give generally great variations; and it is precisely the magnitude of these variations which it is of the utmost importance to learn.

III. What we asserted regarding the temperature in proposition II. applies equally to all meteorological indications; it is of great importance to become acquainted with those variations of the barometer, and of the force and direction of the wind.

IV. The most efficient means for prognosticating the weather are, the employment of the electric telegraphs and of self-registering instruments, because they facilitate and make possible a tabular union of the variations mentioned in II. and III.

In the following remarks these propositions are more fully developed.

Art. I. No one can call in question our first proposition, we shall not therefore demonstrate it; we will only observe, that it is the winds that modify this temperature; we will endeavour to ascertain how much is to be ascribed to each wind.

Let Θ represent for longer, θ for shorter spaces of time, the *mean theoretical temperature* of a place, and let MT and *mt* represent the *mean temperature deduced from long series of observations*, OT and *ot* the *observed mean temperature* for that longer or shorter space of time at the same place in a given year; then we need only annex some distinctive symbols to these three signs in order to show of which space of time we are speaking. Therefore, when we speak of years or seasons, we shall annex the first letters of the words *year*, *y*; *winter*, *w*; *spring*, *sp.*; *summer*, *su.*; *autumn*, *a*, at the foot of the great letters; and when we speak of months, we shall annex the first letter of the name of the month after the small letters. Thus Θy will signify the mean temperature of the year, θf the mean temperature of February, which ought to prevail at a certain place, if that place did not receive warmth from and

impart warmth to the surrounding parts : it will readily be perceived that these values must ever be the same for each place situated in the same latitude and elevation above the sea. On what does it depend that MTy and mtf are not found alike in all the places in the same parallel, but always different in every different place; lower in this, higher in that, than Θy and θf ? If there was a parallel circle which extended wholly over a continent, or if we passed regularly over a parallel circle in a ship, and if we determined the MTy in n different selected places, then would $\frac{\Sigma MTy}{n}$ be nearly expressed by Θy ; the value so obtained would, however, be somewhat greater than Θy , because there is more air drawing towards the north than towards the south over a whole parallel circle, that southern air at the same time being warmer; and also because, near the equator, the latent heat which is employed in the vaporizing of water is greater than that which is freed by rain; and, on the contrary, in higher latitudes there is more freed than expended on the formation of vapour. This is also a cause why on the land Θy must be something less than at sea. If we would nicely calculate this influence, we ought equally to distinguish between rain that is formed in different altitudes. Properly we ought to infer theoretically Θy , and equally so θf , from the warmth which emanates from the sun to us every day; from the warmth which every day and night issues from beneath the surface of earth; and from the warmth that is produced by animals, consumed by plants, lost by radiation, given by condensation of vapour. The difference between Θy so obtained, and MTy , ($MTy - \Theta y$), at a certain place, is to be ascribed to the influence of the wind during the year; similarly, $mtf - \theta f$ is the influence of the wind which prevails in February at that place. How far we are as yet from such a determination may be seen, for example, from the meteorology of the late illustrious Daniell. But even if it could be obtained, it would, however, not be fit for a great space of time; for certain it is, that the resultant of the winds of a whole year would not have the same influence on the temperature as the different components. This will appear more evident to any one who directs his attention to the differences $mty - \theta y$, $mtf - \theta f$, $mtjl - \theta jl$, &c. He would find, not only for every place, but also for every month of the year, different influences for the same wind; the same wind producing a different cooling or heating power in different seasons, a circumstance of which account is not taken in computing the resultant.

Art. II. We will suppose that not only Θ and θ , but also MT and mt were obtained at a certain place from a long series of

observations, that MT therefore could be considered as the equilibrated state of temperature at a determined season of the year at that place; then we should find that the differences $OT - MT$, $ot - mt$ are to be ascribed to these circumstances:— 1st, that the winds had not in that space of time the same direction as usual; and 2nd, that the distribution of temperature at the surrounding places was quite different from the usual distribution during that space of time, or shortly before. Thus it is necessary that we know the variations, not only for the place itself for which we desire to explain the temperature, but also for the surrounding places, since the variations at the first place must be explained partly from the variations at the latter. The most important causes are always to be sought in the variations (deviations); it is from those that we must derive the exhibition of the state of temperature, not from the absolute observed temperature. Even now, when we give absolute temperatures, we do in fact give deviations; namely, deviations from the arbitrary zero of temperature. Positive prognostications will always be given under the form of deviations, and best by deviations from the mean state of the atmosphere. Certainly it were to be wished that we better understood the theoretical temperature (Θ), which must take place for certain soils and elevations above the sea, and for every latitude, in order also to be able to give the deviations from those theoretical values; but for want of that knowledge the deviations from the mean value are the main points for consideration.

Art. III. Temperature is that state which most attracts the attention of the public, in consequence of the immediate influence it has on the relations of human life; but scientifically, all states are equally important. The pressure of vapour is perhaps the most uncertain element, and of which we can give little explanation, from the manner in which it is measured; electricity is not anywhere (with exception of Brussels and Kew) sufficiently known either as to its origin or as to its quantity; it is not therefore expected that the deviations of these elements can be given. It is the winds that bring us the warm air and cold waves; they must be accurately noted as to direction and strength, but it will avail little to give their deviations from the mean direction; the winds must therefore be noted with their real directions. On the contrary, as to the barometer, the deviations again are of the greatest importance, especially as here the theoretical state is known for every latitude. Everywhere, where attention has been paid to the deviations of the barometer, which at the same time have been observed at different places or successively at the same place,

it has led, in connexion with the observation of the wind, to practical discoveries. Who knows not the fruits of the labours of Redfield, Reid, Piddington, Thom? In later times also, the investigations of Mr. Birt on the atmospheric waves, which in November pass over England and Europe, are only deduced from the knowledge of the deviations.

As we must give so much importance to the deviations of the barometer and thermometer as to assert that they are applicable in most meteorological investigations; as we further consider how much trouble it causes for a calculator to subtract the value obtained from every observation which he uses from the mean value at that place for that day, whilst the observer can so very easily, whilst he is noting his observations, note in a column next to it the difference from the mean, we dare propose and urgently invite the observer everywhere, where the mean of each day is sufficiently known, or even where this is known with some approximation, to take this little trouble. We should then, after a lapse of five or ten years, be able to modify the mean of the temperature, and then to determine it with greater certainty. A book that contains such means for every day of the year for many places would certainly be a most useful book; and if one once possessed that, then it would be sufficient for the observers to communicate the deviations; from time to time the corrections could be published in the form of supplements.

Art. IV. England has given the example of communicating the state of the weather* at a certain instant in all parts of the country, at least to Glasgow, by means of telegraph, as I saw to my surprise in the *Greenock Daily News*. My proposition made in the *Changements de Température* was actually in operation. Later I have also learned that that plan is likewise proposed to the British Association: thus the communication of the weather becomes more scientific than it hitherto had been. Telegraphs can, better than self-registering instruments, answer the purpose of approaching to a foretelling of the weather at all places before it exists there; they give us the opportunity of being informed of the weather before it has passed from one place to another. We can be on our guard and arrange our occupations (our observations in the first place) accordingly; remarkable phænomena will be noticed by different observers in corresponding manners at different places, and many other advantages will result. It is true, we shall

* This publication was principally planned and is carried into execution by the proprietors of the *London Daily News*. The report of the state of the weather at 9^h in the morning at numerous places is published every day in that journal.—G. B. AIRY.

be able likewise to see from the graphic self-registering instruments the state of the weather at every instant at all places where they exist (and this is the main point of agreement of both), but only *long after* those circumstances are past; from the telegraphs we are acquainted with it *instantaneously*. It will yet be *some time*, we avow it, before we become sufficiently acquainted with the laws of the transit of the states of the weather, to be able to foretell with any certainty even from the known state of the weather at the neighbouring places (particularly, also, because to the class of "surrounding places" belong the places above us, and the winds and temperature reigning there); but we shall *never* arrive at this point except by means of exhibition of simultaneous observation. When we draw a series of theoretical deductions from a given distribution of meteorological indications (wind, heat, pressure, &c.), and compare them with the subsequent distribution which is really found to follow as an effect of the first, we cannot fail to penetrate better the origin and influence of the winds, and moreover the variations of temperature produced by them.

Therefore it will be of importance to improve our graphic methods of representing simultaneous observations, and either by that means or by numbers to exhibit those states in a tabular form. As to the graphic method, it is to be seen in the articles of Mr. Birt, Report of the Meeting of the British Association, 1844, 1845, &c., that there is a want of a uniform method for noting three or more variations at the same time. A good graphic method would have enabled Mr. Birt to communicate more in fewer pages, and to put every reader in a state to try his conclusions and to extend them.

Martins, in his annotations to the translation of Kämtz, has endeavoured to exhibit more than three variations at the same time; but that method, in our opinion, is not very clear.

Since the investigation of the great November atmospheric wave is probably to be continued, it is perhaps not inconsistent to propose another manner of exhibiting it. We can form a map of all the places where observations are made, and give to each place a sign, for example (*a*), (*b*), &c.; then the position of those signs in the other part of the drawing must express the height of the barometer at those so indicated places in the following manner. Let us draw round that map a circle, which we consider as the section of a vertical cylinder, in the plane of which those places are situated; let us further suppose that at the places (*a*) (*b*) the barometers are placed with the lower surface of the mercury in the same plane of that circular section; so will the top of the highest standing barometer lie in a plane parallel to the first, and give a circular

section of the cylinder; the distance between the two circles will be proportional to the height of the barometer at that place. This will be true for all places; and the height of the letters or signs (*a*) (*b*) noted at the respective sections in the surface of the cylinder would indicate the height of the barometer at those places. If we imagine our eye in the axis of the cylinder, and we project those supposed points (*a*) (*b*) on the horizontal plane, or what is the same thing, if we apply the method of descriptive geometry to this, the greater or lower height of the barometers at (*a*) (*b*) will be represented by points, α , β , in circles concentric with the first, but more or less removed, whose distance from the first (the difference of the radius) will be proportional to that height. So the first circle may represent a very low barometrical height, as 730 millimetres, and each millimetre that the barometer in (*a*) or (*b*) is higher may be represented by one or two millimetres' distance of the circle (α) or (β) from the first. But we can go a step further; we can divide those circles from the common centre point into a number of sectors, thirty sectors I suppose: in one sector the letters may signify the height of the barometer at the 20th of October, in another at the 21st, and so on; every sector may be applied to one day. So we shall have the barometrical range at every one place, and the different states at the different places simultaneously, equally well and clearly represented, while the map within the circle indicates the distribution with respect to the surface of the earth. If, by chance, the points (*a*) in the different sectors lie in a circle, it indicates that the barometer at *a* has not varied during the month; if they lie in an ellipse, then there have been two highest and two lowest states; we see immediately those heights and the dates. So, as to the simultaneous comparison of the barometer heights, the eye catches immediately the letters that are higher or lower, and we see on the map in the middle the line of places where the barometer was at the highest or the lowest. If there actually is a wave, then it is clearly exhibited thus to the eye; if the eye has difficulty in discerning the wave, then probably there is no wave, or perhaps that wave must have been marked out by the foregoing or following days.

It is even possible to go a step further, and, for example, to represent at the same time the thermometer state by the distances at which we set the letters (*a*), (*b*) (the signs of the places) from the radii which divide the circle; the more on the right, for example, the higher the state of the thermometer. It is evident, however, that the more variations we exhibit, the more is lost in simplicity. We could have

represented also latitude, longitude, time, barometer, thermometer. How much soever we improve the graphic representation, we must however always, where there is not merely a view required but an accurate calculation, have recourse to a tabular exhibition. To promote this, the Utrecht Provincial Society for Arts and Sciences has issued the following prize question:—

Since the usual indications of thermometer, barometer, and anemometer throughout the year, that is, the periodical functions by which those indications are expressed, are now known for a great number of places, at least with approximation, and thus a sufficient basis is laid for the investigation of non-periodical changes; the Society wishes—

After having calculated the mean indications of thermometer, barometer and anemometer, over as great a number of places as possible in Europe and Asiatic Russia for periods of five days for two successive years,—

1. The deviations of thermometer and barometer from their above-described usual state in each of those periods, should be united in tables;

2. The manner of transit of those deviations in time and space should be investigated; and

3. Those deviations should be compared and brought in connexion with the winds that prevailed during each of the said periods, and with the deviation from their usual direction which they exhibited.

The importance of this prize proposition in connexion with the various researches which are required for its investigation, has induced the author of this question to augment the offered prize with one hundred and fifty guilders. It is wished that the answer be received before October 1, 1851, by M. C. von Marle, Secretary of the Utrecht Provincial Society for Arts and Sciences. The name of the author, as usual, in a separate letter.

Utrecht.

Postscript.—I have not mentioned in my memoir that the observations instituted by the Royal Society have lately been computed, and that the means of each month are in fact subtracted from the mean of the months of the same name. I had not seen the last part of the Philosophical Transactions in which these computations are placed.

IV. *On the Triadic Arrangements of Seven and Fifteen Things.* By A. CAYLEY, Esq.*

THERE is no difficulty in forming with seven letters, a, b, c, d, e, f, g , a system of seven triads containing every possible duad; or, in other words, such that no two triads of the system contains the same duad. One such system, for instance, is

$abc, ade, afg, bdf, beg, cdg, cef$;

and this is obviously one of six different systems obtained by permuting the letters a, b, c . We have therefore six different systems containing the triad abc ; and there being the same number of systems containing the triads abd, abe, abf and abg respectively, there are in all thirty-five different systems, each of them containing every possible duad. It is deserving of notice, that it is impossible to arrange the thirty-five triads formed with the seven letters into five systems, each of them possessing the property in question. In fact, if this could be done, the system just given might be taken for one of the systems of seven triads. With this system we might (of the systems of seven triads which contain the triad abd) combine either the system

$abd, acg, aef, bce, bfg, dcf, deg$,

or the system

$abd, acf, aeg, bcf, dce, dfg$.

But any one of the other abd systems would be found to contain a triad in common with the given abc system, and therefore cannot be made use of. For instance, the system $abd, acg, aef, bcf, beg, dce, dfg$ contains the triad beg in common with the given abc system; and whichever of the two proper abd systems we select to combine with the given abc system, it will be found that there is no abe system which does not contain some triad in common, either with the abc system or with the abd system.

The order of the letters in a triad has been thus far disregarded. There are some properties which depend upon considering the triads obtained by cyclical permutations of the three letters as identical, but distinct from the triads obtained by a permutation of two letters or inversion. Thus abc, bca, cab are to be considered as identical *inter se*, but distinct from the triads acb, cba, bac , which are also identical *inter se*. I write down the system (equivalent, as far as the mere combination of the letters is concerned, to the system at the com-

* Communicated by the Author.

mencement of this paper)

ade, afg, bdf, bge, cdg, cef, cba,

derived, it is to be observed, from a pair of triads, *ade, afg*, by a cyclical permutation of the *e, f, g*, and by successively changing the *a* into *b* and *c*, the remaining triad of the system being the letters *a, b, c* taken in an inverse order. Let it be proposed to derive the system in the same manner from any other two triads of the system; for instance, from the triads *acb, ade*. The process of derivation gives

*acb, ade, gcd, geb, fce, fbd, fga**,

which is, in fact, the original system. But attempt to derive the system from the two triads *ebg, efc*, the process of derivation gives

ebg, efc, dbf, deg, abc, agf, ade,

which is not the original system, inasmuch as the triads *dbf, deg, abc, agf* are inversions of the triads *bdf, cdg, cba, afg* of the original system. The point to be attended to, however, is, that *both* triads of the pair *dbf, deg*, or of the pair *abc, agf*, are inversions of the triads of the corresponding pair in the original system; the pair is either reproduced (as the pair *efc, dbf*), or there is an inversion of both triads. Where there is no such inversion of the triads of a pair, the system may be said to be properly reproduced; and where there is inversion of the triads of one or more pairs, to be improperly reproduced. There is no difficulty in seeing that the system is properly reproduced from a pair of triads containing in common any one of the letters *a, b, c* or *d*, and improperly reproduced from pairs of triads containing in common any one of the letters *d, e* or *f*. It is owing to the reproduction, proper or improper, of the system from any pair of duads that it is possible to form a system of "octaves" analogous to the quaternions of Sir William Hamilton; the impossibility of a corresponding system of fifteen imaginary quantities arises from the circumstance of there being always, in whatever manner the system of triads is formed, an inversion of a single triad of some one or more pairs of triads containing a letter in common. When the system is considered as successively derived from different pairs, the system is not according to the previous definition reproduced either properly or improperly. A system of triads having the necessary properties with respect to the mere combination of the letters (viz. that $\alpha\beta\gamma$ and $\alpha\delta\epsilon$ being any two triads having a letter in common, there shall be triads such

* The order of the letters *f, g* is selected so as to reproduce the original system so far as the mere combination of the letters is concerned.

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as $\zeta\beta\delta$, $\zeta\gamma\varepsilon$, and $\eta\beta\varepsilon$, $\eta\gamma\delta$) may easily be found; the system to be presently given of the triads of fifteen things would answer the purpose. And so would many other systems.

Dropping the consideration of the order of the letters which form a triad, I pass to the case of a system of fifteen letters, $a, b, c, d, e, f, g, h, i, j, k, l, m, n, o$. It is possible in this case, not only to form systems of thirty-five triads containing every possible duad, but this can be done in such manner that the system of thirty-five triads can be arranged in seven systems of five triads, each of these systems containing the fifteen letters*. My solution is obtained by a process of derivation from the arrangements $ab.cf.dg.eh$ and $ab.cd.ef.gh$ as follows; viz. the triads are

<i>iab</i>	<i>jac</i>	<i>kaf</i>	<i>lad</i>	<i>mag</i>	<i>nae</i>	<i>oah</i>
<i>icf</i>	<i>jfb</i>	<i>kbc</i>	<i>lce</i>	<i>mch</i>	<i>ncd</i>	<i>ocg</i>
<i>idg</i>	<i>jde</i>	<i>kdh</i>	<i>lgb</i>	<i>mbd</i>	<i>ngf</i>	<i>ofd</i>
<i>ieh</i>	<i>jhg</i>	<i>kge</i>	<i>lhf</i>	<i>mfe</i>	<i>nhb</i>	<i>obe</i>

and a system formed with i, j, k, l, m, n, o , which are then arranged—

<i>klo</i>	<i>ino</i>	<i>jmo</i>	<i>ilm</i>	<i>jln</i>	<i>ijk</i>	<i>kmn</i>
<i>iab</i>	<i>jac</i>	<i>lad</i>	<i>nae</i>	<i>kaf</i>	<i>mag</i>	<i>oah</i>
<i>ncd</i>	<i>mdb</i>	<i>kbc</i>	<i>ocg</i>	<i>mch</i>	<i>lce</i>	<i>icf</i>
<i>mef</i>	<i>keg</i>	<i>ieh</i>	<i>jfb</i>	<i>obe</i>	<i>ofd</i>	<i>jde</i>
<i>jgh</i>	<i>lhf</i>	<i>nfg</i>	<i>khd</i>	<i>idg</i>	<i>nhb</i>	<i>lbg</i>

an arrangement, which, it may be remarked, contains eight different systems (such as have been considered in the former part of this paper) of seven letters; viz. of the letters i, j, k, l, m, n, o ; and of seven other sevens, such as i, j, k, a, b, c, f . The theory of the arrangement seems to be worth further investigation.

Assuming that the four hundred and fifty-five triads of fifteen things can be arranged in thirteen systems of thirty-five triads, each system of thirty-five triads containing every possible duad, it seems natural to inquire whether the thirteen systems can be obtained from any one of them by cyclical permutations of thirteen letters. This is, I think, impossible. For let the cyclical permutation be of the letters a, b, c, d, e, f ,

* The problem was proposed by Mr. Kirkman, and has, to my knowledge, excited some attention in the form "To make a school of fifteen young ladies walk together in threes every day for a week so that each two may walk together." It will be seen from the text that I am uncertain as to the existence of a solution to the further problem suggested by Mr. Sylvester, "to make the school walk every week in the quarter so that each three may walk together."

g, h, i, j, k, l, m. Consider separately the triads which contain the letter *n* and the letter *o*; neither of these systems of triads contains the letter, whatever it is, which forms a triad with *n* and *o*. Hence, omitting the letters *n, o*, we have two different sets, each of them of six duads, and composed of the same twelve letters. And each of these systems of duads ought, by the cyclical permutation in question, to produce the whole system of the seventy-eight duads of the thirteen letters. Hence arranging the duads of the thirteen letters in the form

ab . bc . cd . de . ef . fg . gh . hi . ij . jk . kl . lm . ma
ac . bd . ce . df . eg . fh . gi . hj . ik . jl . km . la . mb
ad . be . cf . dg . eh . fi . gj . hk . il . jm . ka . lb . mc
ae . bf . cg . dh . ei . fj . gk . hl . im . ja . kb . lc . md
af . bg . ch . di . ej . fk . gl . hm . ia . jb . kc . ld . me
ag . bh . ci . dj . ek . fl . gm . ha . ib . jc . kd . le . mf

and consequently the duads of each set ought to be situated one duad in each line. Suppose the sets of duads are composed of the letters *a, b, c, d, e, f, g, h, i, j, k, l*, it does not appear that there is any set of six duads composed of these letters, and situated one duad in each line, other than the single set *al, bk, cj, di, eh, fg*; and the same being the case for any twelve letters out of the thirteen, the derivation of the thirteen systems of thirty-five triads by means of the cyclical permutations of thirteen letters is impossible. And there does not seem to be any obvious rule for the derivation of the thirteen systems from any one of them, or any *prima facie* reason for believing that the thirteen systems do really exist, it having been already shown that such systems do not exist in the case of seven things.

2 Stone Buildings, June 14, 1850.

V. *On some remarkable effects of Lightning, observed in a Farm-house near Moniemail, near Cupar-Fife. Communicated by WILLIAM THOMSON, Esq., M.A., Fellow of St. Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow*.*

THE following is an extract from a letter, addressed last autumn to me, by the Rev. Mr. Leitch, minister of Moniemail parish:—

“ Moniemail Manse, Cupar-Fife,
 August 26, 1849.

“ * * We were visited on the 11th inst. with a violent thunder-storm, which did considerable damage to a farm-

* Read before the Philosophical Society of Glasgow, Dec. 5, 1849.

house in my immediate neighbourhood. I called shortly afterwards and brought away the wires and the paper which I enclose. * *

“I have some difficulty in accounting for the appearance of the wires. You will observe that they have been partially fused; and when I got them first, they adhered closely to one another. You will find that the flat sides exactly fit. They were both attached to one crank, and ran parallel to one another. The question is, how were they attracted so powerfully as to be compressed together? * *

“You will observe that the paper is discoloured. This has been done, not by scorching, but by having some substance deposited on it. There was painted *wood* also discoloured, on which the stratum was much thicker. It could easily be rubbed off, when you saw the paint quite fresh beneath. * *

“The farmer showed me a probang which hung on a nail. The handle only was left. The rest, consisting of a twisted cane, had entirely disappeared. By minute examination I found a small fragment which was not burnt but broken off.”

[The copper wires, and the stained paper enclosed with Mr. Leitch's letter, were laid before the Society.]

The remarkable effects of lightning described by Mr. Leitch, are all extremely interesting. Those with reference to the copper wires are quite out of the common class of electrical phænomena; nothing of the kind having, so far as I am aware, been observed previously, either as resulting from natural discharges, or in experiments on electricity. It is not improbable that they are due to the electro-magnetic attraction which must have subsisted between the two wires during the discharge, it being a well-known fact that adjacent wires, with currents of electricity in similar directions along them, attract one another. It may certainly be doubted whether the inappreciably short time occupied by the electrical discharge could have been sufficient to allow the wires, after having been drawn into contact, to be pressed with sufficient force to make them adhere together and to produce the remarkable impressions which they still retain. On the other hand, the electro-magnetic force must have been very considerable, since the currents in the wires were strong enough nearly to melt them; and, since they appear to have been softened, if not partially fused, the flattening and remarkable impressions might readily have been produced by even a slight force subsisting after the wires came in contact.

The circumstances with reference to the probang, described by Mr. Leitch, afford a remarkable illustration of the well-known fact, that an electrical discharge, when effected through the substance of a non-conducting (that is to say, a *powerfully resisting*) solid, shatters it without producing any considerable elevation of its temperature, not leaving any marks of combustion, if it be of an ordinary combustible material such as wood.

Dr. Robert Thomson, at my request, kindly undertook to examine the paper removed from the wall of the farm-house, and enclosed with his letter to me by Mr. Leitch, so as if possible, by the application of chemical tests, to discover the staining substance deposited on its surface. Mr. Leitch, in his letter, had suggested that it would be worth while to try whether this case is an example of the deposition of sulphur, which Fusinieri believed he had discovered in similar circumstances. Accordingly tests for sulphur were applied, but with entirely negative results. Stains presenting a similar appearance had been sometimes observed on paper in the neighbourhood of copper wires, through which powerful discharges in experiments with the hydro-electric machine had been passed; and from this it was suggested that the staining substance might have come from the bell-wires. Tests for copper were accordingly applied, and the results were most satisfactory. The front of the paper was scraped in different places, so as to remove some of the pigment in powder, and portions of the powder from the stained and from the not stained parts were separately examined. The presence of black oxide of copper in the former was readily made manifest by the ordinary tests; in the latter no traces of copper could be discovered. The back of the paper presented a green tint, having been torn from a wall which has probably been painted with Scheele's green; and matter scraped away from any part of the back was found to contain copper. Since, however, the stains in front were manifestly superficial, the discoloration being entirely removed by scraping, and since there was no appearance whatever of staining at the back of the paper, nor of any effect of the electrical discharge, it was impossible to attribute the stains to copper produced from the Scheele's green on the wall below the paper. Dr. Thomson, therefore, considered the most probable explanation to be, that the stains of black oxide of copper must have come from the bell-wire. To ascertain how far this explanation could be supported by the circumstances of the case, I wrote to Mr. Leitch asking him for further particulars, especially

with reference to this point, and I received the following answer :—

“ Moniemail, Cupar-Fife,
Nov. 30, 1849.

“ * * I received your letter to-day, and immediately called at Hall-hill, in the parish of Colessie, the farm-house which had been struck by the lightning. * *

“ I find that Dr. Thomson’s suggestion is fully borne out by the facts. I at first thought that the bell-wire did not run along the line of discoloration, but I now find that such was the case.” * *

[From a drawing and explanation which Mr. Leitch gives, it appears that the wire runs vertically along a corner of the room, from the floor, to about a yard from the ceiling, where it branches into two, connected with two cranks near one another and close to the ceiling.]

“ The efflorescence [the stains previously adverted to] was on each side of this perpendicular wire. In some places it extended more than a foot from the wire. The deposit seemed to vary in thickness according to the surface on which it was deposited. There was none on the plaster on the roof. It was thinnest upon the wall-paper, and thickest upon the wood facing of the door*. This last exhibited various colours. On the thickest part it appeared quite black; where there was only a slight film, it was green or yellow. * *

“ I may mention that the thunder-storm was that of the 11th of August last. It passed over most of Scotland, and has rarely been surpassed for terrific grandeur,—at least beyond the tropics. It commenced about 9 o’clock P.M., and in the course of an hour it seemed to die away altogether. The peals became very faint, and the intervals between the flashes and the reports very great, when all at once a terrific crashing peal was heard which did the damage. The storm ceased with this peal.

“ The electricity must have been conducted along the lead on the ridge of the house, and have diverged into three streams; one down through the roof, and the two others along the roof to the chimneys. One of these appears to have struck a large stone out from the chimney, and to have been conducted down the chimney to the kitchen, where it left traces upon the floor. It had been washed over before I saw it, but still the traces were visible on the Arbroath flags.

* These remarkable facts are probably connected with the conducting powers of the different surfaces. The plaster on the roof is not so good a conductor as the wall-paper with its pigments; and the painted wood is probably a better conductor than either.—W. T.

The stains were of a lighter tint than the stone, and the general appearance was as if a pail of some light-coloured fluid had been dashed over the floor, so as to produce various distinct streams. All along the course of the discharge, and particularly in the neighbourhood of the bell-wires, there were small holes in the wall about an inch deep, like the marks that might be made by a finger in soft plaster.

“Most of the windows were shattered, and all the fragments of glass were on the outside. I suppose this must be accounted for by the expansion of the air within the house.

“The window-blind of the staircase, which was down at the time, was riddled, as if with small shot. The diameter of the space so riddled was about a foot. On minute examination I found that the holes were not such as could readily be made by a pointed instrument or a pallet. They were angular, the cloth being torn along both the warp and the woof.

“The house was shattered from top to bottom. Two of the servant maids received a positive shock, but soon recovered. A strong smell of what was supposed to be sulphur, was perceived throughout the house, but particularly in the bedroom, in which the effects I described before took place.”

VI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxxvi. p. 549.]

Feb. 7, “**O**N the development and homologies of the Molar Teeth 1850. of the Wart-Hogs (*Phacochærus*), with illustrations of a System of Notation for the Teeth in the Class Mammalia.” By Richard Owen, Esq., F.R.S. &c.

The author commences by a brief statement of the facts and conclusions recorded in a paper by Sir Ev. Home on the dentition of the *Sus Æthiopicus*, in the Philosophical Transactions for 1799, p. 256; and gives the results of an examination of the original specimens described and figured by Home, and of other specimens showing earlier stages of dentition, which lead to the following conclusions as to the number, kinds, and mode of succession of the teeth in the genus *Phacochærus*. The tooth answering to the first milk-molar and first premolar in the upper jaw, and those answering to the first and second milk-molars and corresponding premolars in the lower jaw of the common Hog are not developed. Eight successive phases of development of the grinding teeth of the African Wart-hogs are described and expressed by the following notation:—

Phase.	No. of grinding teeth.	Kinds of teeth.
I.	$\frac{5-5}{4-4}$ viz.	$\left\{ \begin{array}{l} d\ 2, d\ 3, d\ 4, m\ 1, m\ 2. \\ d\ 3, d\ 4, m\ 1, m\ 2. \end{array} \right.$
II.	$\frac{6-6}{5-5}$ viz.	$\left\{ \begin{array}{l} p\ 2, p\ 3, p\ 4, m\ 1, m\ 2, m\ 3. \\ p\ 3, p\ 4, m\ 1, m\ 2, m\ 3. \end{array} \right.$
III.	$\frac{5-5}{4-4}$ viz.	$\left\{ \begin{array}{l} p\ 3, p\ 4, m\ 1, m\ 2, m\ 3. \\ p\ 4, m\ 1, m\ 2, m\ 3. \end{array} \right.$
IV.	$\frac{4-4}{4-4}$ viz.	$p\ 4, m\ 1, m\ 2, m\ 3.$
V.	$\frac{4-4}{3-3}$ viz.	$\left\{ \begin{array}{l} p\ 3, p\ 4, m\ 2, m\ 3. \\ p\ 4, m\ 2, m\ 3. \end{array} \right.$
VI.	$\frac{3-3}{3-3}$ viz.	$p\ 4, m\ 2, m\ 3.$
VII.	$\frac{2-2}{2-2}$ viz.	$p\ 4, m\ 3.$
VIII.	$\frac{1-1}{1-1}$ viz.	$m\ 3.$

These observations prove that, contrary to the opinion of Home and Cuvier, the Wart-hogs have deciduous teeth, succeeded vertically by premolar teeth; in the *Phacochærus Æliani*, at least, three deciduous teeth are, in some individuals, succeeded by as many premolar teeth; and, as a general rule, two deciduous teeth are displaced vertically by two premolars. The first true molar is remarkable for its unusually early development, which is followed by an unusually early abrasion and expulsion, when its place is obliterated by the second true molar being pushed forwards into contact with the last premolar. This tooth is as remarkable for its longevity, and remains after the wearing away and shedding of the second true molar, when the last true molar advances into contact with the last premolar, and the place of both the previously intervening true molars is obliterated. This unusual order of shedding of the molar teeth has given rise to the idea of the last large and complex true molar of the *Phacochærus* being the homologue of both the last and penultimate grinders of the common Hog, which the author's observations refute; and he, also, is able to point out, by re-examination of the original specimen figured by Home in the Phil. Trans., the source of the erroneous idea that the common Hog had an additional true molar behind the large one symbolised by $m\ 3$, in the author's system of dental notation.

The nature and signification of the symbols proposed are explained and illustrated by a series of drawings. One of the fruits of the determination of the homology of a part is the power of giving it a name, and signifying it by a symbol applicable co-extensively with such homology. The limits are shown within which the homologies of individual teeth can be determined: they present

the requisite constancy of character in a large proportion of the class Mammalia. Certain members of this class, *e.g.* the order *Bruta* and the *Cetacea vera*, have teeth too numerous and alike in form and mode of development to admit of being determined individually from species to species. Such mammalia have but one set of teeth, and the author proposes to call them 'Monophyodonts.' On the other hand, the orders *Marsupialia*, *Insectivora*, *Rodentia*, *Ruminantia*, *Pachydermata*, *Carnivora*, *Cheiroptera*, *Quadrumania* and *Bimana* have two sets of teeth, and might be called collectively, 'Diphyodonts.' Of the permanent teeth of this division of mammalia, some succeed the deciduous teeth vertically, others come into place behind one another in horizontal succession. The 'incisors' are determined by a character of relative position to the jaws and to each other: so likewise the 'canines.' The remaining teeth are divided into those which are developed in vertical relation to the deciduous molars, and push them out, and those that have not such relation, but follow each other horizontally: the term 'molar' is restricted by the author to these latter teeth, and that of 'premolar' to the former ones, which are always anterior to the molars. There is a remarkable degree of constancy in the number of these different kinds of teeth; in the placental Diphyodonts, *e.g.* the 'incisors'

never exceed $\frac{3-3}{3-3}$, *i.e.* 3 on each side of both jaws, the 'canines'

$\frac{1-1}{1-1}$, the premolars $\frac{4-4}{4-4}$, the molars $\frac{3-3}{3-3}$, =44; and this

the author regards as the typical formula of dentition in the great proportion of the mammalian class above defined. It was rarely departed from by the primæval species that have become extinct, and is modified chiefly by defect or loss of certain teeth in the existing species. When the grinders are below the typical number, the missing molars are taken from the back part of their series, and the premolars from the fore part of theirs: the most constant teeth being the fourth premolar and first true molar; these are always determinable, whatever be their form, by the relation to them of the last tooth of the deciduous series. Thus determined, the homologies of the other grinders are ascertained by counting the molars from the first backwards, 1, 2, 3; and the premolars from the last forwards, 4, 3, 2, 1. The symbols are made by adding the initial *m* to the numbers of the molar teeth, and the initial *p* to those of the premolar teeth. The author concludes by pointing out the advantages of this system of anatomical notation.

2. "Description of the Hydrostatic Log." By the Rev. E. L. Berthon, M.A. Communicated by Sir Francis Beaufort, F.R.S. &c., on the part of the Lords Commissioners of the Admiralty.

The object of this invention is to obtain a register of the speed of ships, by a column of mercury, in such a manner that the height of the column shall depend upon the velocity alone, and not be affected by any disturbing causes, such as alteration of draught of water, pitching and rolling, &c.

The principle embraces that of Pitôt's tube, inasmuch as the

force of the resistance due to the velocity is communicated through a small pipe projecting into the water below the bottom of the ship: this force, acting upwards, compresses a portion of enclosed air in a small cylinder, which air communicating by means of a little pipe with the bulb of a glass tube—bent like a common barometer—raises the mercury in the tube, by depressing it in the bulb.

But as any single column of water and air thus acting upon the surface of the mercury in the *bulb alone* must depend not only upon the resistance due to the velocity, but also upon the *distance of the cylinder from the water-line*, which distance or height varies with every sea, and alters more permanently as the draught of water changes, a compensation was necessary; and the inventor has found one, which he considers perfect for all these variations, by applying *a second column of water and air* to press upon the *other surface of the mercury*, viz. that in the glass tube. This second column is precisely like the first as regards the pipe and cylinder, and communicates with the sea by an aperture or apertures, presented in such a direction that velocity does *not produce any increase of pressure*. Thus the mercury in the indicator is placed *between two columns* of water and air, which are *always equal to each other* in length, and the mercury rises according to the *difference* between the pressures upon its two surfaces, the result of resistance or velocity alone.

The air-pipes may be conducted in any direction, and the indicator, which swings upon gimbals, may be placed in any part of the ship. The two water-pipes are conducted into one tube in the bottom of the ship, divided into two separate chambers for the different forces.

In addition to the speed, the true course or leeway of the vessel is indicated upon a horizontal segment divided into degrees, over which a needle is moved by a rod connected with the above-mentioned double tube; and the whole is kept continually in the true direction of the ship's motion by a float or vane attached to the lower end of the tube in the water.

Feb. 14.—“Supplementary Observations on the Structure of the Belemnite and Belemniteuthis.” By Gideon Algernon Mantell, Esq., LL.D., F.R.S., Vice-President of the Geological Society, &c.

In this communication the author describes his recent investigations on the structure of the two genera of fossil Cephalopoda, whose remains occur so abundantly in the Oxford clay of Wiltshire, namely, the Belemnite and Belemniteuthis, as supplementary to his memoir on the same subject, published in the Phil. Trans. 1848. In that paper evidence was adduced to show the correctness of the opinion of the late Mr. Channing Pierce as to the generic distinction of these two extinct forms of Cephalopoda.

As however several eminent naturalists had expressed doubts as to some of the opinions advanced by the author in his former memoir, figures and descriptions are given in the present notice, of beautiful and instructive specimens lately discovered in Wiltshire, and which he conceives establish his previous conclusions. Dr. Mantell then states as the result of his examination of several hundred examples,

that our knowledge of the organization of the animal of the Belemnite is at present limited to the following parts, viz.—

1. An external *Capsule* or *periostracum* which invested the osselet or sepistaire, and extending upwards, constituted the external sheath of the receptacle.

2. The *Osselet*, characterized by its fibrous radiated structure, terminating distally in a solid rostrum or guard, having an alveolus, or conical hollow, to receive the apical portion of the chambered phragmocone; and expanding proximally into a thin cup, which became confluent with the capsule, and formed the receptacle for the viscera.

3. The *Phragmocone*, or chambered, siphunculated, internal shell; the apex of which occupied the alveolus of the guard, and the upper part constituted a capacious chamber, from the basilar margin of which proceeded two long, flat, testaceous processes.

These structures comprise all that are at present known of the animal to which the fossil commonly called "*The Belemnite*," belonged.

Of the *Belemnoteuthis*, the fossil cephalopod which Prof. Owen regards as identical with the Belemnite, many examples of the body with eight uncinated arms, and a pair of long tentacula, having an ink-bag and pallial fins, have been discovered. The osselet of this animal, like that of the Belemnite, has a fibro-radiated structure, investing a conical chambered shell; but this organ, for reasons fully detailed in the memoir, the author considers could never have been contained within the alveolus of a Belemnite; the soft parts of the animal of the Belemnite are therefore wholly unknown.

Many beautiful specimens of Belemnites and Belemnoteuthis were exhibited by Dr. Mantell to the Society, in proof of the statements contained in the memoir.

2. "On the PELOROSAURUS; an undescribed gigantic terrestrial reptile, whose remains are associated with those of the Iguanodon and other Saurians, in the Strata of Tilgate Forest." By Gideon Algernon Mantell, Esq., LL.D., F.R.S., Vice-President of the Geological Society, &c.

The author had for a long while entertained the idea, that among the remains of colossal reptiles obtained from the Wealden strata, there were indications of several genera of terrestrial saurians, besides those established by himself and other geologists. The recent discovery of an enormous arm-bone, or humerus, of an undescribed reptile of the crocodilian type, in a quarry of Tilgate Forest in Sussex, where Dr. Mantell had many years since collected numerous teeth and bones of the Iguanodon, Hylæosaurus, &c., and some remarkable vertebræ not referable to known genera, induced him to embody in the present communication the facts which his late researches have brought to light.

The humerus above-mentioned was found imbedded in sandstone, by Mr. Peter Fuller of Lewes, at about 20 feet below the surface; it presents the usual mineralized condition of the fossil bones from the arenaceous strata of the Wealden. It is four and a half feet in

length, and the circumference of its distal extremity is 32 inches ! It has a medullary cavity 3 inches in diameter, which at once separates it from the *Cetiosaurus* and other supposed marine saurians, while its form and proportions distinguish it from the humerus of the *Iguanodon*, *Hylæosaurus*, and *Megalosaurus*. It approaches most nearly to the *Crocodylians*, but possesses characters distinct from any known fossil genus. Its size is stupendous, far surpassing that of the corresponding bone even of the gigantic *Iguanodon* ; and the name of *Pelorosaurus* (from *πέλωρ pelor*, monster) is therefore proposed for the genus, with the specific term *Conybeari*, in honour of the palæontological labours of the Dean of Llandaff.

No bones have been found in such contiguity with this humerus, as to render it certain that they belonged to the same gigantic reptile ; but several very large caudal vertebræ of peculiar characters, collected from the same quarry, are probably referable to the *Pelorosaurus* ; these, together with some distal caudals which belong to the same type, are figured and described by the author.

Certain femora and other bones from the oolite of Oxfordshire, in the collection of the Dean of Westminster, at Oxford, are mentioned as possessing characters more allied to those of the *Pelorosaurus*, or to some unknown terrestrial saurian, than to the *Cetiosaurus*, with which they have been confounded.

As to the magnitude of the animal to which the humerus belonged, Dr. Mantell, while disclaiming the idea of arriving at any certain conclusions from a single bone, states that in a *Gavial* 18 feet long, the humerus is 1 foot in length ; *i. e.* one-eighteenth part of the length of the animal, from the end of the muzzle to the tip of the tail. According to these admeasurements the *Pelorosaurus* would be 81 feet long, and its body 20 feet in circumference. But if we assume the length and number of the vertebræ as the scale, we should have a reptile of relatively abbreviated proportions ; even in this case, however, the original creature would far surpass in magnitude the most colossal of reptilian forms.

In conclusion, Dr. Mantell comments on the probable physical conditions of the countries inhabited by the terrestrial reptiles of the secondary ages of geology. These highly-organized colossal land saurians appear to have occupied the same position in those ancient faunas as the large mammalia in those of modern times. The trees and plants whose remains are associated with the fossil bones, manifest, by their close affinity to living species, that the islands or continents on which they grew possessed as pure an atmosphere, as high a temperature, and as unclouded skies as those of our tropical climes. There are therefore no legitimate grounds for the hypothesis in which some physiologists have indulged, that during the "*Age of Reptiles*" the earth was in the state of a half-finished planet, and its atmosphere too heavy, from an excess of carbon, for the respiration of warm-blooded animals. Such an opinion can only have originated from a partial view of all the phenomena which these problems embrace, for there is as great a discrepancy between the existing faunas of different regions, as in the extinct groups of animals and plants which geological researches have revealed.

The memoir was illustrated by numerous drawings, and the gigantic humerus of the Pelorosaurus and other bones were placed before the Society.

Feb. 21.—“On the Extension of the Principle of Fermat’s Theorem of the Polygonal Numbers to the higher orders of series whose ultimate differences are constant. With a new Theorem proposed, applicable to all the Orders.” By Sir Frederick Pollock, Lord Chief Baron, F.R.S.

The object of this paper professes to be to ascertain whether the principle of Fermat’s theorem of the polygonal numbers may not be extended to all orders of series whose ultimate differences are constant. The polygonal numbers are all of the *quadratic* form, and they have (according to Fermat’s theorem) this property, that every number is the sum of not exceeding, 3 terms of the triangular numbers, 4 of the square numbers, 5 of the pentagonal numbers, &c.

It is stated in this paper that the series of the odd squares 1, 9, 25, 49, &c. has a similar property, and that every number is the sum of not exceeding 10 odd squares. It is also stated, that a series consisting of the 1st and every succeeding 3rd term of the triangular series, viz. 1, 10, 28, 35, &c., has a similar property; and that every number is the sum of not exceeding 11 terms of this last series, and that this may be easily proved [it was proved in a former paper by the same author]. The term “Notation-limit” is applied to the number which denotes the largest number of terms of a series necessary to express any number; and the writer states that 5, 7, 9, 13, 21 are respectively the notation-limits of the tetrahedral numbers, the octohedral, the cubical, the eicosahedral and the dodecahedral numbers; that 19 is the notation-limit of the series of the 4th powers; that 11 is the notation-limit of the series of the triangular numbers squared, viz. 1, 9, 36, 100, &c., and 31 the notation-limit of the series 1, 28, 153, &c. (the sum of the odd cubes), whose general expression is $2n^4 - n^2$.

The paper next contains an extension of the theorem $8n + 3 = 3$ odd squares, which was proved by Legendre in his *Théorie des Nombres*; every odd square equals 8 times a triangular No. + 1; the theorem therefore is—8 times any term in the figurate series (1, 2, 3, 4, &c. ..) + 3 = 3 terms of a series consisting of the next series, viz. (1, 3, 6, 10 . . &c.), multiplied by 8 with 1 prefixed, and also added to each term. But it is stated that this theorem may be much extended; for this is not only true of any two consecutive series, but generally if F_x represent any figurate number of the x^{th} order, and F_y any figurate number of the y^{th} order, whether y be greater or less than x ,

$$8F_x + 3 = 3, \text{ or } (3 + 8), \text{ or } (3 + 2 \cdot 8), \text{ or } \dots (3 + n8), \text{ \&c.,}$$

terms of a series whose general expression is $8F_y + 1$; and still further (provided p be greater than 2)—

$$pF_x + 3 = 3, \text{ or } (3 + p), \text{ or } (3 + 2p), \text{ or } (3 + np),$$

terms of a series whose general expression is $pF_y + 1$, and *vice versa*.

The author concludes from these considerations, that probably

there are many theorems which are common to all the orders. The following theorem is then proposed as having that character.

If the terms of a series be

$$\begin{aligned}
 &1, \text{ or } (1+n)^0, (1+n)^1, (1+n)^2 \dots \&c. (1+n)^p, \\
 &\quad \text{the 1st } (p+1) \text{ terms of } (1+n)^{p+1} \\
 &\quad \text{the 1st } (p+1) \text{ terms of } (1+n)^{p+2} \\
 &\quad \text{the 1st } (p+1) \text{ terms of } (1+n)^{p+3} \\
 &\quad + \&c. \&c.
 \end{aligned}$$

(if p and n be both not less than 1), any number will be the sum of not exceeding $(pn+1)$ terms of the series; in other words, $pn+1$ is the notation-limit of this series.

It is manifest that this series is of such a form, that by varying n and p , it is capable of expressing every possible arithmetical series, also every possible geometrical series (each having 1 for the first term); it will also express all the intermediate series of the successive orders (to an indefinite extent), which exist between and connect together by a regular gradation (as is well known) any such arithmetical series with a geometric series, whose common ratio is the 2nd term of both series. The theorem may be stated without the series thus:—

If any geometric series (having 1 for its first term) and $(1+n)$ for its common ratio, be stayed at the $(p+1)$ th term and discontinued as a *geometric* series, but be continued from that term as an *arithmetic* series of the p th order, by forming it with the p th difference as the constant difference, and the other differences (which will be $x, x^2, x^3, \&c. \dots x^p$). The resulting series will be the series stated in the theorem above, and any number may be formed by not exceeding $(pn+1)$ terms, that is $(pn+1)$ will be the notation-limit of the series; if p becomes indefinitely great, the limit of the series is a geometrical series, and it would become capable of expressing any number according to a system of notation whose base or local value would be $(1+n)$.

The proof of the theorem seems to depend upon this, that the notation-limit assigned by the theorem is actually the notation-limit of all the geometric terms and one more, at least, while the geometric terms alone fix the law of the series and ascertain its *elements* (that is, the first term and the successive differences); and as the combinations necessary to enable the series to fulfill its law, and carry on the notation that belongs to it, are regulated by the series next below it, viz. by the first rank of differences, while the supply of new combinations (as the series advances and the number of terms that may be used increases) is indicated by even a higher series than itself, the new combinations are always greater, and at length indefinitely greater, than the number required. If therefore within the range of those terms that ascertain and fix the law of the series the law of its notation-limit can be obeyed, it must always (*à fortiori*) be obeyed as the series proceeds to a greater number of terms and to a variety of combinations increasing in a higher ratio; and the

series will furnish the numbers requisite to carry on the notation by the new and more numerous combinations which must of necessity be of the same numerical kind with those which have preceded them. It is shown at length, that the new combinations, as the series advances, do actually increase in an increasing proportion compared with the numbers required.

2. "Experiments on the section of the Glossopharyngeal and Hypoglossal Nerves of the Frog, and Observations of the alterations produced thereby in the Structure of their primitive fibres." By Augustus Waller, M.D. Communicated by Professor Owen, F.R.S. &c.

After describing the natural structure of the tubular fibres of the nerves, the author states the results which he observed to follow the section of the nerves of the frog's tongue. To this organ two principal pairs of nerves are distributed; one of these, issuing from the cranium along with the pneumogastric and distributed to the fungiform papillæ, is regarded as the glossopharyngeal; the other, arising from the anterior part of the spinal cord, and passing through the first intervertebral foramen, the author (following Burdach) names the hypoglossal. Section of the glossopharyngeal nerves does not cause any perceptible loss of motion or of common sensation, and this fact, together with its distribution to the fungiform papillæ, leads the author to consider this nerve as the nerve of tasting. On the other hand, when the hypoglossal nerves are divided, the tongue is no longer sensible to mechanical irritation, and its motions are entirely abolished. Simultaneous division of the right and left glossopharyngeal nerves is followed by the death of the animal in a few days, and the same effect ensues after division of both hypoglossals. This result, which takes place more speedily in summer than in winter, the author is disposed to ascribe to a disturbance of the mechanical process of respiration, in which, as is well known, the muscles of the frog's mouth and tongue take a large share.

To ascertain the changes which take place in the nerve-fibres after division of the trunks to which they belong, the operation was confined to the nerve of one side only, and the fibres of the uninjured nerve of the other side served for comparison. These changes ensue more speedily, and go on more rapidly in summer than in winter, commencing usually in about five days. The pulp contained in the tubular nerve-fibres, originally transparent, becomes turbid, as if it underwent a sort of coagulation, and is soon converted into very fine granules, partly aggregated into small clumps, and partly scattered within the tubular membrane. These granules are at first abundant, and render the nerve-fibre remarkably opaque; but in process of time they diminish in number, and, together with the inclosing membrane, at length disappear, so that at last the finest ramifications of the nerves which go to the papillæ, or those going to the muscular fibres of the tongue (according to the nerve operated on), are altogether lost to view, in consequence of the destruction and eva-

nescence of their elementary fibres. The disorganization begins at the extremities of the fibres, and gradually extends upwards in the branches and trunk of the nerve. The other tissues of the tongue remain unaltered. When the cut ends of the nerve are allowed to reunite, the process of disorganization is arrested, and the nervous fibres are restored to their natural condition. The author ascribes the disorganization and final absorption of the nerve-fibres to an arrestment of their nutrition caused by interruption of the nervous current, and considers his experiments as affording most unequivocal evidence of the dependence of the nutrition on the nervous power.

Feb. 28.—“Sequel to a Paper on the reduction of the Thermometrical Observations made at the Apartments of the Royal Society, with an Appendix.” By James Glaisher, Esq., F.R.S.

The principal object of this paper is the connexion of the results deduced in a former paper from the observations at the Royal Society's Apartments, with the observations at the Royal Observatory at Greenwich, in order to determine mean numerical values, and to establish the laws of periodic variation from this long series of observations; the two series of observations are here reduced to one and the same series.

The observations at the Royal Society having been discontinued between the years 1781 and 1786, it was necessary to supply this link in the series, more particularly as these years were distinguished by very severe weather, and their omission would have a sensible effect on the results. The deficient observations have been supplied by a comparison of the observations which were made at Somerset House, with the observations during the corresponding years made by Mr. Barker at Lyndon in Rutlandshire, from 1771 to 1799, corrections being thus obtained for reducing the Lyndon observations to those at Somerset House.

By a comparison of the temperature of the air at Somerset House and at the Royal Observatory for every month during the years 1833 to 1843, corrections necessary to be applied for reducing the mean values at the one place to those at the other, are deduced.

Thus the results of the observations at Somerset House are reduced to those at the Royal Observatory, and a table is given showing the mean temperature at the latter place of each month in every year from 1771 to 1849 inclusive. By taking the means of the several columns in this table, the mean temperature of each month is deduced from all the observations. From these mean monthly temperatures a table is constructed showing the excess of the mean monthly temperature at Greenwich for each year, above the temperature of the month deduced from all the years.

Tables are next given showing the mean temperature in quarterly periods for the year, and for successive groups of years at the Royal Observatory at Greenwich, from the year 1771 to 1849; and the excess of the quarterly and yearly mean temperatures in every year, and for groups of years, above the means from all the years. The author remarks that the numbers in these tables do not at all confirm the idea that a hot summer is either preceded or followed by a cold

winter, or *vice versâ*; but on the contrary it would appear that any hot or cold period has been mostly accompanied by weather of the same character, and instances are cited in support of this conclusion.

Tables are also given, based upon the readings of the self-registering thermometers, exhibiting the extreme readings at Somerset House and at the Royal Observatory.

Incidentally the author goes into an inquiry respecting the relative temperature of London and the country in its neighbourhood. From the observations made by Mr. Squire at Epping from 1821 to 1848, and also from those at Lyndon, he concludes that the general fact of a higher winter temperature and lower summer temperature at the Royal Society's Apartments is satisfactorily proved, and that the same cause has been in operation at both seasons; this cause he considers to be the vicinity of the river Thames to the place of observation. With the view of showing the extent to which this cause operates, a table is given of the mean monthly temperature of the water of the Thames, and a comparison is made between the results of observations made on board the 'Dreadnought' Hospital Ship, at the height of 32 feet above the water, with simultaneous observations at the Royal Observatory. From this comparison it is concluded, that at all seasons of the year the temperature at the 'Dreadnought' is above that at the Observatory during the night hours, and that it is below during the mid-day hours only: at times of extreme temperature the effects of the water upon the temperature of the air is very great.

The paper is accompanied by diagrams exhibiting to the eye, by means of coordinates, the numerical results given in the tables.

2. "On the Communications between the Tympanum and Palate in the Crocodilian Reptiles." By Richard Owen, Esq., F.R.S. &c.

After citing the descriptions by Cuvier, Kaup, Bronn, and De Blainville of the Eustachian tubes and the foramina in the base of the cranium of the recent and extinct Crocodiles, the author gives an account of the nerves, arteries, veins and air-tubes that traverse these different foramina, and thus determines the true position of the carotid foramina and posterior nostrils in the *Teleosauri* and other fossil *Crocodylia*, which had been a matter of controversy amongst the authors cited. In the course of these researches the author discovered a distinct system of Eustachian canals superadded to the ordinary lateral Eustachian tubes, which he describes as follows:—

"From each tympanic cavity two passages are continued downwards, one expands and unites with its fellow from the opposite side to form a median canal which passes from the basisphenoid to the suture between that and the basioccipital, where it terminates in the median canal continued to the orifice described by M. De Blainville as the posterior nostril. The second passage leads from the floor of the tympanic cavity to a short canal which bends towards its fellow, expands into a sinus and divides: one branch descends and terminates in the small lateral foramen at the lower end of the suture between the basioccipital and the basisphenoid: the other branch continues the course inwards and downwards until it meets its fellow at the median line of the basioccipital, and it forms the posterior

primary division of the common median canal: this soon joins the anterior division, and the common canal terminates at the median opening below. Membranous tubes are continued from the three osseous ones, and converge to terminate finally in the single Eustachian orifice on the soft palate behind the posterior nostril. The mucous membrane of the palate lines the various osseous canals above described, and is continued by them into the lining membrane of the tympanum."

With regard to the homologies of the above described air-passages, the author states that the lateral canals answer to the simple Eustachian tubes of Lizards and Mammals, and that the median canal, with its dichotomous divisions, is a speciality peculiar to the Crocodilian reptiles.

The memoir was illustrated by nine drawings of the size of nature.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xxxvi. p. 240.]

March 11, 1850.—On the Numerical Calculation of a class of Definite Integrals and Infinite Series. By Professor Stokes.

In a paper "On the Intensity of Light in the neighbourhood of a Caustic," printed in the sixth volume of the Cambridge Philosophical Transactions, Mr. Airy, the Astronomer Royal, has been led to consider the integral

$$W = \int_0^{\infty} \cos \frac{\pi}{2} (w^3 - mw) dw,$$

and has tabulated it from $m = -4$ to $m = +4$ by the method of quadratures. In a supplement to the same paper, printed in the fifth part of the eighth volume, Mr. Airy has extended the table as far as $m = \pm 5.6$, by means of a series proceeding according to ascending powers of m . This series, though convergent for all values of m , however great, is extremely inconvenient for numerical calculation when m is large, and moreover gives no information as to the law of the progress of the function for large values of m . The author has obtained the following expression for the calculation of W for large, or even moderately large, positive values of m :

$$W = 2(3m)^{-\frac{1}{3}} \left\{ R \cos \left(\phi - \frac{\pi}{4} \right) + S \sin \left(\phi - \frac{\pi}{4} \right) \right\},$$

where

$$R = 1 - \frac{1.5.7.11}{1.2(72\phi)^2} + \frac{1.5.7.11.13.17.19.23}{1.2.3.4(72\phi)^4} - \dots,$$

$$S = \frac{1.5}{1.72\phi} - \frac{1.5.7.11.13.17}{1.2.3(72\phi)^3} + \dots,$$

$$\phi = \pi \left(\frac{m}{3} \right)^{\frac{2}{3}}.$$

When m is negative, and $+mw$ is written for $-mw$ in the integral W , so that in the altered form of the integral m is positive, there results

$$W = 2^{-\frac{1}{3}}(3m)^{-\frac{1}{3}\epsilon - \phi} \left\{ 1 - \frac{1.5}{1.72\phi} + \frac{1.5.7.11}{1.2(72\phi)^2} - \dots \right\}.$$

By means of these expressions, W may be calculated with great facility when m is at all large. The author has given a table of the roots of the equation $W=0$, from the second to the fiftieth inclusively, calculated by a formula derived from the former of the above expressions. This formula was not sufficiently convergent to give the first root to more than three places of decimals; but this root may be obtained more accurately from Mr. Airy's table.

The method by which the author has treated the integral W appears to be of very general application, and he has further exemplified it by applying it to the infinite series

$$1 - \frac{x^2}{2^2} + \frac{x^4}{2^2 \cdot 4^2} - \frac{x^6}{2^2 \cdot 4^2 \cdot 6^2} + \dots = \frac{2}{\pi} \int_0^{\frac{\pi}{2}} \cos(x \cos \theta) d\theta,$$

which occurs in a great many physical investigations, as well as to the integral which occurs in investigating the diffraction produced by a screen with a small circular aperture, placed in front of the object-glass of a telescope through which a luminous point is viewed.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxxvi. p. 477.]

Jan. 11, 1850.—An account of some Improvements in a Speculum Grinding and Polishing Machine. By John Hippisley, F.R.A.S.

“The machine, as far as regards the general action of two eccentric pins, which transmit motion to the grinding and polishing tools, is similar in principle to that adopted by Lord Rosse, and other machines previously in use.

“The improvements consist in the arrangement of the parts so as to effect cheapness and facility in the construction, with general convenience in use, and *especially* in the manner in which the polishing tool is connected with the apparatus which gives it motion.

“This connexion is made by a ball and socket-joint of a novel construction, which, while it transmits a perfectly equable motion, without jerks or irregularities, to the polisher, and leaves it free also to revolve about its own centre, as the friction between it and the speculum may direct, facilitates the application of counterpoise weights, so as to counterbalance in any required degree the weight of the polisher, especially in the very last period of the polishing.

“A polisher of considerable lightness has justly been deemed indispensable, and for this purpose wood has been used instead of metal in its construction. This material is, however, obviously liable to unsymmetrical alteration of figure, from unequal expansion and contraction by moisture and heat.

“Nor does it, it is submitted, adequately fulfill the condition required, namely, that of sufficiently removing pressure from the speculum. It appears almost demonstrable that the last finishing action of the polisher will be exerted with most advantage, to the perfection both of figure and polish, when it moves without any vertical pressure whatever. Those who are familiar with the adjustment of reflecting telescopes are aware that the slightest confinement or pressure on the speculum, when in its place in the telescope, is sufficient to impair its power of definition, in other words, to alter temporarily, in

some degree, however small, its figure. A pressure, certainly far less than that which a wooden polisher will exert, is capable of producing this injurious effect.

“It is, then, to be expected, that when a speculum has been receiving its final figure under any pressure, there will be some, however slight, recession from that figure when the pressure is removed; and if so, the remedy obviously is to diminish *successively*, by counterpoises, the weight of the polisher as the figure and polish are advancing, till it ultimately moves, in giving the last finishing strokes, without any more pressure than the cohesion between the polished and sliding surfaces affords. It may also be expected that the lustre and perfection of the polish itself will be enhanced, inasmuch as the size of the abraded atoms will then be at their minimum, since they diminish in a direct ratio with the diminution of pressure by the abrading surface.

“In the machine to which these observations refer, the pulley which drives one excentric rod is made to differ slightly in diameter from its fellow, with which it is connected by the endless band, which gives motion to the other excentric rod; so that the centre of the polisher, by their joint action, is constrained to describe curves, varying from nearly a straight line to ellipsoids having a minor axis equal to twice the thrust of the shorter excentric; and the parabolic figure is attained by the maintenance of a certain proportion, found experimentally, between the throws of these two excentrics. This proportion, for a speculum having an aperture of one inch for each foot of focal length, and under the action of a polisher of the same dimensions as the speculum, is one-third of the diameter of the speculum for the longer, and one-seventh for the shorter thrust; the figure of the speculum receding towards a spherical figure if that shorter thrust be diminished, or advancing to the hyperbolic curvature if it be increased.

“The counterpoises, then, it is suggested, should be added successively, and at those periods in the action of the machine when it may be considered that its tendency to give a parabolic figure is at its maximum; that is, when, by the combined action of the excentric pins, the centre of the polisher is describing the widest ellipsoids on the speculum. A bent wire is so placed, as an index, on the excentric rod, that its point, traversing a scale fixed in any convenient position, shows the exact moment when these widest ellipsoids are being described: at those intervals the counterpoise-weights are successively added, so that the polisher may be considered to pass through *complete* cycles of its action under each alteration and diminution of pressure.

“Attention to these conditions has, in specula finished by the machine, apparently been entirely successful, both in obtaining an exceeding fine polish, and a figure which does not sensibly deviate from the parabolic.

“A nice attention to the quantity and purity of the water used in the polishing is also of much importance. A very convenient method, which, being self-acting, requires no particular care during the process, is to fix a small vessel on the excentric rod, with a thread de-

pending from it, and so adjusted, that the single drop, which by the capillary and syphon action passes from the thread, shall, at the extreme thrust of the excentric rod, be deposited at the edge of the speculum. The quantity may be nicely regulated by the use of a smaller or larger thread; and the water is secure from containing any gritty particle, being filtered in its passage through the thread.

“The machine is of an easy and cheap construction, and intended to be worked by the foot acting on a treadle, or by any other convenient motive power: the same lever apparatus which is used for counterpoising the polisher in finishing, being connected by a vertical rigid bar with the ball and socket-joint of the tool, affords means for adding any required amount of pressure in the rough grinding process, and thus that tedious part of the operation is considerably accelerated. It is also adapted for figuring lenses of large dimensions, to which it would impart, as well as to specula, surfaces approaching nearly to those of parabolic, or other required geometric curvatures, and with the addition of another mandril, or more, receiving rapid motion from the periphery of the fly-wheel, it has been used with great convenience for grinding and polishing lenses of the smallest dimensions.”

BRITISH METEOROLOGICAL SOCIETY.

We have to notice the recent formation of a Society for the advancement of Meteorological knowledge, “a branch of physical inquiry which,” as stated in the published Address, “requires the combined efforts of numerous observers, steadily following a well-concerted plan, employing the same class of instruments, and reducing their results in the same form.

“Amongst the objects of this Society will be the reduction of observations and combination of results, as far as their funds will allow; nevertheless it is to be hoped that the emulation which will naturally be excited amongst observers to supply observations producing the best results, will also induce them to reduce their own observations, as far as they may be able to do so. With the view of stimulating observers to perform this work, this Society will publish from time to time useful Tables to facilitate the reduction of observations.

“The Council fully trust, that whilst the establishment of the British Meteorological Society will be the means of diffusing throughout this country a philosophical spirit of inquiry, and of inducing a more general employment of trustworthy instruments, and the carefully noting and submitting for comparison the observations thus made, it will also have a more extended influence. By facilitating a comparison of the observations of its own members with those made in other countries, meteorological phenomena will be better traced, and thus effects more satisfactorily and surely referred to their true causes. A remarkable instance of the want of connecting observations has been so recently rendered obvious, that it may not be without its use briefly to refer to it here. In the ‘American Traveller,’ published at Boston on April 6, 1850, and recently received in this country, is a paper by W. Cranch Bond, Esq., of Cam-

bridge, United States, in which he speaks of the great atmospheric wave which was passing over England from the 1st to the 18th day of February 1849, the mean reading of the barometer during this interval of time being fully half an inch above its average value, and when the crest of the wave was over Greenwich, the reading of the barometer at the level of the sea was 30.90 inches. The base of the wave at this time seems to have been in extent just equal to the distance from England to America; for on the same day that it completed its passage at Greenwich, it was first felt at Boston, and it was seventeen days passing over Boston, as it was with us. Its motion, therefore, must have been about 170 miles daily. The reduced readings of the barometer during the time of the passage of the wave at Boston, and its extreme readings, were identical in value with those at Greenwich. At present we cannot follow this very remarkable heaping up of the air from the want of observations at different places.

“Another object of this Society will be to avail itself of every opportunity of establishing observatories in those parts of the world where none are at present in existence.

“Other beneficial results to be expected from a Society of this kind are, the diffusion of a spirit of inquiry concerning the use of instruments, the practice and extension of meteorological researches, and the encouragement of mutual information among its members.”

The number of Members already exceeds one hundred; and the List of Officers for the year is the following:—

President.—Samuel Charles Whitbread, Esq., F.R.A.S.

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Council.—Capt. Francis Blackwood, R.N., F.R.A.S.; Rev. Professor T. Chevallier, B.D., F.R.A.S.; John Drew, Esq., F.R.A.S.; Vincent Fasel, Esq., F.R.A.S.; Rev. Samuel King, M.A., F.R.A.S.; George Leach, Esq., F.Z.S.; Edward Joseph Lowe, Esq., F.R.A.S.; Rev. Charles Lowndes, M.A., F.R.A.S.; Rev. Joseph Bancroft Reade, M.A., F.R.S.; William Rutter, Esq., F.R.A.S.; Thomas Shapter, M.D.; Professor John Stevelly, LL.D.

VII. *Intelligence and Miscellaneous Articles.*

THE LAGOONS OF TUSCANY.

THE Tuscan Lagoons are, properly speaking, natural depressions of the soil ordinarily filled with water from which hot vapours are ejected. They are situated within a space of ten or twelve miles, lying between $28^{\circ} 27'$ and $28^{\circ} 40'$ of longitude, and between $43^{\circ} 10'$ and $43^{\circ} 15'$ of latitude. The principal lagoons are those of Monte Cerboli, of Castel Nuovo in the valley of Cecina, those of Sasso, of Monte Rotondo, of the Lago del Edifizio, of Lustignano and of Serazzano in the valley of Cornia. The ancients were acquainted

with the Tuscan lagoons, and the name of Mount Cerberus accords well with the poetical and mythological ideas of the early people of Italy.

Even as late as the 18th century the lagoons were regarded only as a supernatural wonder which excited astonishment rather than courted investigation. Under the Grand Duke Leopold I., the chemist Hœfer discovered by analysis, that they contained boracic acid. This discovery, followed by further explorations, has bestowed upon the lagoons an unrivalled industrial importance, and has brought into the countries possessing them an activity contrasting strikingly with the miserable state in which they before languished. It is a curious fact in their history that before the discovery of this acid, the foetid odour developed by the sulphuretted hydrogen gas,—the certain death which met the man who fell into the scalding baths,—the disruptions of the ground occasioned by the appearance of new *Soffioni*,—and above all the superstitious terror with regard to them, had made the people consider the lagoons as a scourge from which they sought deliverance by public prayers; but now, if by any cause the *Fumacchi*, the source of common prosperity, should become extinguished, they would not fail to seek from heaven a restoration of this scourge, which in the skilful hands of M. Larderel, has become, to quote M. Bowring, a source perhaps of greater riches than the mines of Peru or of Mexico, and certainly more reliable. After the discovery of Hœfer, Paul Mascagni, a noted chemist, had the first idea of procuring from the lagoons boracic acid like that of China, and of thus restoring to Europe the tribute that she had paid to Asia. But the attempt was not at first profitable, as the waters contain in solution at the moment of their escape from the earth, only an insignificant quantity of boracic acid. Another chemist having observed that a part of the acid was thrown beyond the lagoon, by the violence of the vapours, and that it was scattered on the margins of the craters, and moreover being confident that the waters were capable of dissolving a greater quantity of acid, endeavoured to find means of saturating them by constructing upon the declivities of the country artificial lakes fed by the streams from the mountain. The vapours which issue from these lakes keep their waters constantly at a boiling temperature. After impregnation for twenty or thirty hours by the vapours of the highest lake, they draw off the waters into the second lake to submit them to a new impregnation. From thence they are drawn into a third, and so on till they reach the receptacle at the lowest point. In their passage across six or eight lakes, they are charged with half per cent. of boracic acid. They are then led into the reservoir from which they are conducted into lead reservoirs for evaporation, to produce concentration; and to hasten that operation, the happy idea was proposed of substituting for the combustibles sometimes used, and which were enormously expensive, the direct application of the heat of the *Soffioni*. This improvement decided the success of the enterprise. It is surprising that it was introduced at so late a day, since this method was not new and had been long practised at the solfatara of Pozzuoli in extracting alum from the earth that contains it. In the lagoons, the hot vapours for carrying on the evaporation are taken

at their origin and carried across by lead pipes or by subterranean conduits below the boilers. Thus the fabrication is extremely simple, the locality itself furnishing the means of carrying it on. A single discharge of the vapours is sufficient to throw into ebullition, almost immediately, 20 or 30 caldrons of a capacity of 20 barrels, which may be estimated at 84,000 pounds of liquid impregnated with boracic acid. Before allowing the vapours to escape, they direct them under the ovens in order to free the acid from its hygrometric moisture. Of late the somewhat complex system of boilers and coolers has been simplified by substituting rectangular tables of lead of 20 or 30 metres, divided at small intervals by transverse parallel divisions, but whose height is never raised above that of the edges. These tables have an inclination of two to three degrees. The water of the last lagoon is introduced upon the upper side, in small quantities. The hot vapours for evaporating are conducted in such a manner that they act upon the lower surface. The liquid after having filled the first compartment is diffused very gradually into the second, then into the third, and so successively to the last, where it reaches such a state of concentration that it deposits the crystallized acid; the workmen remove it immediately by means of wooden scrapers. This mode of gradual concentration is very ingenious, and requires so few hands that it may almost be said that the acid is obtained without expense. From 1818 to 1845 the quantity of acid manufactured was 33,349,097 Tuscan pounds. From 1839 to 1845 the mean quantity has been two millions and a half of pounds.

Thus in estimating the product at 7500 pounds per day, the quantity of saturated water upon which they operate daily is 1,500,000 lbs. daily, and annually 547,500,000 lbs.

This labour brings to Tuscany 12 millions of pounds (10 millions of francs), and it is surprising that it should have remained unproductive during so many ages, and that it should have been reserved for the skill of M. Larderel, now Count of Monte Cerboli, and before 1818 a simple wandering merchant, entirely unacquainted with scientific researches, to discover the fugitive vapours and render them a source of inexhaustible wealth.

The violence with which the burning vapours escape gives rise to muddy explosions, when a lake has been drained by turning its waters into another lake. The mud is then thrown out, as solid matters are ejected from volcanos, and there forms in the bottom of the lake a crowd of those little cones of eruption whose activity and play recall exactly under another form the *hornitos* of Malpays. Their temperature varies from 120° to 145° centigrade, and the clouds which they form above the lagoons constitute true natural barometers, whose greater or less density rarely disappoints the predictions that they announce.

While in an industrial point of view, the lagoons occupy the first rank among the natural products of Tuscany, they place new resources at the disposition of science, permitting the investigation of various geological phenomena, even under the direction of the will of the experimenter. The metamorphic gypsum which we have seen produced at Pereta under the influence of sulphuretted hydrogen vapours, is formed at the lagoons, which, like those of Monte Cerboli

and of Castel Nuovo, are made to cross argillaceous limestone beds, and with such abundance that their formation may be fully tested. Action also takes place at the same time upon the walls of fractures and the fissures of the soil which open a passage to the subterranean vapours. Thence it extends gradually into the interior of the masses, and it ends by gypsifying whole circles whose radius is generally that of the lagoons themselves. Pure limestones are converted into a lamellar sulphate of lime, but of a loose texture and free of cel- lules. This structure is probably due to the expansion they undergo from the addition of new materials, and perhaps also by the passage of the gas at the moment of the crystallization of the salt. The cal- careous formations below the argillaceous, preserve after their trans- formation their primitive position, and they present an alternation of gypseous beds, and of argillaceous beds which the acid has freed from the soluble bases. When this influence is exerted in the direc- tion of the thickness of the strata, it is very common to see towards the limits where the metamorphic influence ceases, a mass of rock strikingly calcareous at one of its extremities, terminating at the other extremity in a gypsum which the inhabitants use for buildings. The resemblance to the gypsum beds, occurring in the midst of the secondary formations, is exhibited even in the reddish tint with which oxidation marks the associate clays of the alberèse. But a peculiarity which has given me the solution of a problem which had embarrassed me thus far, deserves mention; for we have reproduced here certain phænomena of which the enormous deposits of the Pro- vençal Alps present many examples. I had noticed at Roquevaire and at Digne, irregular argillaceous incrustations in which are found entangled without order, angular fragments of sulphate of lime of various sizes. In admitting the transformation of the jurassic lime- stone of these countries posterior to its consolidation under the in- fluence of the acid vapours, it was difficult to explain the mode of formation of these breccias and the manner in which these fragments were introduced. In all these cases they seemed to indicate an overflow of waters, but the theory opposes the intervention of waters for the accomplishment of the facts relative to the conversion of the limestone, or it leaves in doubt the part which they must have acted. But, observe what is apparent at the lagoons of Monte Cerboli and of Castel Nuovo. At the same time that the limestone is changed into gypsum, by the contact of sulphurous agents, the fragments of alberèse which waters had brought down from heights above to the midst of the miry and boiling lakes, are thus changed into sulphate of lime, and constitute, with the clays in which they sink, brecciated argillo-gypseous beds without stratification. That this fact should be equally apparent in the ancient beds under analogous circum- stances, is at least what might be inferred from the examination of that which passes in the lagoons. We should also observe the analogous positions of the *boracite* of Luneburg, which is found in crystals disseminated in gypsum intercalated in the midst of a cretaceous bed, and the boracic acid and borates of the Tuscan lagoons.

These different facts well confirmed, establish in my view an inti-

mate resemblance between the gypsum of the lagoons and the abnormal gypsum beds of secondary regions.

If the silicification of the Macigno which we have noticed in the neighbourhood of the solfatara of Pereta should appear an exaggerated application of the theory brought forward, the verification of it may be traced in the lagoons of Sasso, where the solution of the silix of the freestone and its redeposition are manifest in all places where circumstances allow of this double transformation. The Fumacci of Sasso rise, to the south of the establishments, from beneath a vast mantle of fine-grained freestone, over which passes the mountain road connecting the valley of the Cornia with the Province of Sienna. At intervals the road is interrupted by isolated boiling pools or shallow cavities, which exert a metamorphic action upon the region which they traverse. The first evidence of alteration is apparent in the colour of the rock, which from blackish-gray becomes white. It is cracked in all directions. The vapours follow quickly these lines of separation, attack the silica of the macigno, dissolving it out, and immediately depositing it under a gelatinous form. The gelatinous mass becomes opaque in the air and assumes the resin-like appearance peculiar to hydrated silica. In connexion with this we observe imbedded in a siliceous cement, nuclei of a white micaceous sandstone unaltered at the centre, causing a breccia appearance. This kind of breccia is finally, by the complete solution of the nuclei, converted into a grayish rock entirely siliceous, which resounds under the hammer like clink-stone, and resembles exactly by its aspect and its roughness of touch, porcelain biscuit. Sometimes the solution is more rapid, and then the rock is formed of an agglutination of little grains analogous to those of an ancient quartz rock and possessing its tenacity and hardness. Examined with a glass, each grain is composed of an independent particle or driblet of hydrated silica, and they seem to have collected as viscous tears, such as would have adhered together in hardening. Breislak observed at the solfatara of Pozzuoli fragments of decomposed lava bound together by a siliceous substance almost vitreous; but in the lagoon of Sasso the solution and permanent regeneration of silica, effected at the expense of the macigno, are carried on upon a vast scale and over a space of great extent.—*Bull. Soc. Géol. de France*, Dec., 1848, 147; and Silliman's *American Journal of Science* for May, 1850.

ON THE INTERPRETATION OF MARIOTTE'S LAW. BY LIEUT.
E. B. HUNT, U.S. CORPS OF ENGINEERS.

It is readily demonstrated that in any entirely *homogeneous* medium, the component parts of which act on each other by forces varying as any function of the distance, Mariotte's law must prevail. Both elastic tension and cohesive force will necessarily vary as the density, in a medium assumed as homogeneous, quite irrespective of the law of force, the variation being expressed in terms of distance between the component parts of the medium. Whether the force be attractive or repulsive, varying inversely with the first or hundredth power of the distance, the result is the same; that *entire homogeneity makes Mariotte's law necessary*.

To prove this: assume a perfectly homogeneous medium whose parts exert forces varying as any function of the distance. Assume in this an origin of coordinates, three coordinate axes, X , Y and Z , and three constant elementary distances, dx , dy , dz . Conceive each axis graduated by laying off its element successively from the origin outward. Through each point of graduation on either axis pass a plane parallel to the other axes: do this for each axis. The space around the origin is thus divided into elementary parallelopipeds, each of which contains a like portion of the homogeneous medium.

The force of elastic tension or of cohesion is measured by the resultant action on a unit of surface of the plane X , Y , by all the forces acting in the positive direction of the axis Z , between the parts on opposite sides of the plane X , Y . This resultant is balanced by an equal one acting in the negative direction of the axis Z . To make up this resultant, a certain number of the elementary portions of the medium conspire. It may therefore be equated with a series, each term of which expresses the positive component along the axis Z , of the force exerted between two elementary portions of the medium on opposite sides of the plane X , Y .

If now the density of the medium be varied, each term of this series will vary in the same ratio, since the quantity of matter in each elementary volume varies as the density. The density thus governs each term of the series, by fixing the quantity of matter in each elementary volume. If we call the ratio of the varying density to a standard density N , each term of the series contains N as a simple factor; or the whole series varies as N . Hence the resultant or entire elastic tension or cohesion varies as N , or as the density. This result is entirely independent of any particular law of relation between the forces and distances; and will always be true so long as the elementary volumes can be assumed as homogeneous. As dx , dy , dz can always be taken indefinitely less than the radius of sensible activity of any assumed force, the demonstration can only fail by the parts failing to be homogeneous.

It will be seen by the above, that any inference of the law of repulsive force between ultimate atoms or molecules, cannot be correctly drawn from Mariotte's law, for this leaves the primary forces involved wholly indeterminate. We are by no means authorized to conclude, that in elastic fluids, where the pressure varies as the density, the molecules repel each other directly as the distance.

The demonstration now given has a singular bearing on the atomic theory of material constitution. We know experimentally that Mariotte's law does not prevail uniformly in elastic media, while in liquids and solids it has no show of application. Hence we are bound to infer *non-homogeneousness*. Now how can homogeneousness be interrupted, except through something like an atomic constitution of media? A laminated, filamental, or molecular structure alone can produce heterogeneousness. The two first would confer special properties in certain directions, which are not found in fact. Hence a molecular constitution of matter seems entailed as an inference, from the bare fact that Mariotte's law is not universal. According to the view now presented, the elasticity of gases varies as the density, because the quantity of matter within the sphere of

sensible activity varies in that ratio. As they approach the point of liquefaction, other considerations derived from the special atomic constitutions of the media must be introduced. The entire absence of a limit to the division of parts would produce that homogeneity from which Mariotte's law becomes an inevitable inference. Such an inference, as applied to media in general, being contrary to the fact, a limit to actual division of parts must be admitted. *Any other theory than one of ultimate molecules, separated by spaces, seems to impose inferences conflicting with facts, throwing us back irresistibly into the theory of true molecular structure.*—Silliman's *American Journal*, May, 1850.

Boston, April, 1850.

EFFECTS OF ATMOSPHERIC ELECTRICITY UPON THE WIRES OF THE MAGNETIC TELEGRAPH.

The *Revue Scientifique* for December last contains an interesting article by M. Baumgartner on the subject of the effects of atmospheric electricity upon the wires of the magnetic telegraph. The following are the most interesting of his results:—

1. The needle rarely coincides with the point which is determined by its astatic state and the tension of its suspension thread; almost always it deviates more or less from this point, which proves that it is influenced by an electric current.

2. The variations are of two kinds; there are some which reach 50° , others extend over $\frac{1}{2}^\circ$ or $8'$. The first are less frequent; they differ so often in direction and intensity that it is impossible to deduce a law for them. On the contrary, the small deviations appear connected by a very simple law.

The observations made at Vienna and at Gratz appear to show that, during the day, the electric currents move from Vienna and from Gratz to Semmering, which is more elevated. This direction is inverse during the night. It appears that this change of direction takes place after the rising and setting of the sun.

3. The regular current is less disturbed by the irregular currents when the air is dry and the sky is serene, than when the weather is rainy.

4. In general, the current is more intense with short than with very long conductors; often, even, the current of the longer chain is opposed to the current of the shorter chain.

Where there is a difference of intensity, this difference is far greater than that which could originate from the resistance of the longer conductor.

When the sky is cloudy and the weather stormy, there are frequently observed in the electric conductor currents which are sufficiently intense to affect the telegraphic indicators, which are, however, far from having extreme sensitiveness.

When they were placing the conducting wires of the Northern Telegraph line from Vienna, the workmen frequently complained of a kind of spasms which they felt in handling the wires. These spasms ceased as soon as they took the precaution not to touch the wires with naked hands. These spasms were most frequent and intense in Styria, the highest region of the line. Thus, near Kranichfeld, a workman received a shock sufficiently violent to overturn him and paralyse his right arm.

The action of the atmospheric electricity on the telegraphs is stronger on the approach of a storm; and not unfrequently the wires themselves, and the poles which support them, are destroyed by electric discharges.

M. Baumgartner cites several examples in support of what has just been said. On the 17th of August 1849, a storm which had burst forth at Ollmutz extended to Frielitz, that is to say, to a distance of ten miles. A workman employed at this latter station, in putting up the wires experienced a shock which overturned him, and he experienced a real burn of the fingers which touched the wire. At this time the sky was perfectly serene at Frielitz.—*From the Journal of the Franklin Institute for April 1850.*

METEOROLOGICAL OBSERVATIONS FOR MAY 1850.

Chiswick.—May 1. Cloudy and cold. 2. Fine: clear: frosty. 3. Clear: very dry air: overcast: sharp frost at night. 4. Fine: showery. 5. Cloudy: some angular hail at 6 P.M. 6. Constant rain. 7, 8. Drizzly. 9. Heavy clouds: fine: clear. 10. Clear: cloudy. 11. Fine. 12. Slight shower: fine. 13. Fine: very dry air: rain at night. 14. Cloudy and fine. 15. Fine: cloudy: clear and frosty. 16. Fine. 17. Overcast. 18. Foggy: rain: cloudy. 19. Very fine: cloudy. 20. Uniformly overcast: fine: clear. 21. Fine: cloudless: overcast: rain. 22. Rain: clear at night. 23. Cloudy: clear. 24. Slight fog: dry haze. 25. Cloudy: fine: showery. 26. Showery: overcast. 27. Cloudy: overcast. 28. Fine: showery: clear. 29. Cloudy and fine. 30. Foggy: dry haze: clear. 31. Fine: slightly clouded.

Mean temperature of the month 51°·14

Mean temperature of May 1849 55 ·19

Mean temperature of May for the last twenty-three years . 54 ·22

Average amount of rain in May 1·84 inch.

Boston.—May 1. Cloudy. 2, 3. Fine. 4. Cloudy: rain early A.M. 5. Cloudy: rain P.M. 6. Cloudy: rain A.M. 7, 8. Rain: rain A.M. and P.M. 9. Rain: rain A.M. 10, 11. Cloudy. 12, 13. Fine. 14. Rain A.M. and P.M. 15. Rain: rain A.M. 16. Fine. 17—19. Cloudy. 20. Rain: rain A.M. 21. Cloudy. 22. Cloudy: rain A.M. 23. Fine. 24. Fine: rain P.M. 25, 26. Cloudy: rain A.M. 27. Cloudy: rain A.M. and P.M. 28. Fine. 29. Cloudy: rain P.M. 30. Fine. 31. Cloudy.

Applegarth Manse, Dumfries-shire.—May 1. Slight frost: very cold east wind. 2. Slight frost: wind changed to west P.M. 3. Frost still: slight shower P.M. 4. Cold and ungenial: one sharp shower. 5. Frost: fall of snow: hills white. 6. Frost: clear and cold. 7. Frost hard: is this May? 8. Cloudy A.M.: hail: rain P.M. 9. Frost hard again: most unseasonable. 10. Heavy rain: cleared P.M. 11. Rain in the night: slight shower A.M. 12. Occasional sharp showers. 13. Cold: fair and clear. 14. Fair and clear: keen and cold P.M. 15. Frost again: hail: keen and cold. 16. No frost: cloudy: mild. 17. Fine: cloudy: mild. 18. Fine: air feels moist. 19. Shower in the night: cold east P.M. 20. Parching cold east wind. 21. Warm and sultry: change great. 22. Very warm: thunder and heavy rain. 23. Very warm: thunder: a few drops. 24. Very warm: fair and fine. 25. Soft rain all day: genial and growing. 26. Soft rain all day: blessed change of weather. 27. Rain: fair P.M. 28, 29. Fair throughout: fine. 30. Fine: thunder: shower. 31. Fine: thunder: a few drops.

Mean temperature of the month 49°·1

Mean temperature of May 1849 50 ·5

Mean temperature of May for the last twenty-eight years ... 51 ·1

Average rain in May for twenty years 1·69 inch.

Sandwich Manse, Orkney.—May 1. Clear: fine. 2. Fine: clear. 3. Showers: sleet-showers. 4. Hail: snow-showers. 5. Snow: snow-showers. 6. Clear: drops. 7. Clear: showers. 8. Damp: clear. 9. Frost: clear: cloudy. 10. Cloudy: drops. 11. Showers: hail-showers. 12. Showers: sleet-showers. 13. Bright: rain: clear. 14. Clear: rain: clear. 15. Bright: cloudy. 16. Damp. 17. Fine. 18. Cloudy: fog. 19. Hazy. 20. Bright. 21. Bright: showers: fog. 22. Fine: fog. 23. Cloudy. 24. Hazy: fog. 25. Hazy: rain. 26. Hazy. 27. Cloudy: fine. 28—31. Bright: fine.

Days of Month.		Barometer.				Thermometer.				Wind.		Rain.							
		Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick.											
1850.	May.	Max.	Min.	8½ a.m.	9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	
1.		30.078	30.048	29.67	30.10	30.11	30.26	30.24	51	28	45.5	51½	37½	44½	41	ne.	n.	n.	
2.		30.274	30.197	29.82	30.18	30.13	30.18	30.09	56	23	45	52	31½	44	45½	sw.	nw.	w.	
3.		30.271	30.115	29.82	30.07	29.90	29.94	29.90	61	41	49	53	32½	45	39	sw.	nw.	w.	
4.		29.979	29.728	29.55	29.90	29.62	29.81	29.74	60	32	49	50	35½	39	34	sw.	nw.	w.	
5.		29.617	29.583	29.35	29.56	29.66	29.74	29.84	60	38	50	42	32½	41½	35	n.	sw.	n.	
6.		29.581	29.553	29.26	29.70	29.69	29.78	29.68	45	41	44	49	30½	41½	45	ne.	ne.	ne.	
7.		29.497	29.448	29.30	29.68	29.53	29.67	29.58	48	44	47	52½	28	48	45	e.	se-e.	se.	
8.		29.573	29.369	29.00	29.37	29.42	29.42	29.55	51	39	44	41	38	44	35	n.	n-w.	nw.	
9.		29.933	29.742	29.40	29.74	29.78	29.70	29.63	54	39	41	50	29	40	42	ne.	nw.	s.	
10.		29.997	29.959	29.55	29.54	29.60	29.36	29.43	60	43	44	53½	39½	50	42	sw.	w.	w.	
11.		29.981	29.952	29.49	29.65	29.62	29.43	29.47	62	47	53	53½	43	45½	43	sw.	w.	w.	
12.		30.002	29.946	29.47	29.69	29.88	29.42	29.88	65	35	53	53	37½	42	40½	nw.	w.	nw.	
13.		30.109	30.082	29.69	30.09	30.04	30.06	29.93	62	33	50	53½	36	43	42½	ne.	nw.	w.	
14.		30.082	29.916	29.60	29.87	29.90	29.98	30.04	59	41	47.5	54½	42	44	40	sw.	w.	n-ne.	
15.		29.916	29.902	29.55	29.94	29.90	30.05	29.91	52	25	42	51	30½	45	45	ne.	n-ne.	n-ne.	
16.		29.947	29.930	29.57	29.81	29.78	29.75	29.83	55	37	46	58	40	48	46½	n.	nw.	nw.	
17.		29.927	29.877	29.36	29.63	29.74	29.86	29.76	66	43	58.5	60½	46½	52	47	nw.	calm	sw.	
18.		29.782	29.767	29.36	29.63	29.60	29.65	29.70	68	40	57	58	47	52	47	nw.	w.	sw.	
19.		29.797	29.766	29.43	29.67	29.74	29.87	30.00	72	44	60	58½	45½	50	47	e.	se.	se.	
20.		29.745	29.692	29.37	29.80	29.71	30.12	30.05	69	46	48.5	54	42½	53½	48½	e.	n.	ne.	
21.		29.749	29.684	29.38	29.72	29.66	29.96	29.82	68	49	52	66	46½	51½	48	ne.	n.	w-e.	
22.		29.592	29.519	29.17	29.52	29.40	29.64	29.59	64	44	52	68½	52	58½	48½	ne.	calm	e.	
23.		29.518	29.467	29.08	29.44	29.50	29.59	29.63	72	41	61	68	45	51½	50	w.	se.	se.	
24.		29.461	29.387	29.00	29.43	29.40	29.61	29.61	67	50	61.5	67	50½	50	46½	e.	ne.	e.	
25.		29.563	29.506	28.97	29.30	29.23	29.55	29.36	65	48	62	63	47½	51	48½	sw.	ssw.	e.	
26.		29.716	29.614	29.05	29.19	29.28	29.27	29.27	64	46	60	54½	49	50	49	sw.	ssw.	s.	
27.		29.737	29.692	29.22	29.45	29.52	29.36	29.57	64	46	61	62	49½	55	52	sw.	s.	s.	
28.		30.167	29.980	29.43	29.70	29.94	29.70	29.93	63	42	60.5	63	50	55	49	sw.	w.	sw.	
29.		30.221	30.137	29.70	30.00	30.00	29.98	30.04	72	45	64	63	50½	56½	52½	s.	sw.	s.	
30.		30.050	30.026	29.60	29.98	30.00	30.07	30.10	70	46	60	66	51	58½	52	e.	e.	se.	
31.		30.201	30.090	29.68	30.09	30.14	30.15	30.22	76	41	61	69	48½	57	54	nw.	s.	se.	
Mean		29.873	29.796	29.42	29.730	29.723	29.771	29.786	61.97	40.32	52.5	56.7	41.4	48.62	45.17	1.84	1.86	1.75	1.84

THE
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VIII. *On the Aërometric Balance, an instrument for measuring the Density of the Air in which it is situated.* By Professor POTTER, A.M., late Fellow of Queen's College, Cambridge*.

THAT the density of the air is an important element in every discussion of atmospheric phenomena, will at once be admitted; but it will be often maintained also, that we have the means of determining it from observations of the wet and dry bulb thermometers and the barometer. A little reflection will, however, soon convince us that this is not absolutely true, since it supposes a regular constitution of the atmosphere for all localities and all states of the weather, which certainly does not exist. Thus, in a room where many persons have been for some time assembled, the carbonic acid gas coming from their lungs in respiration will increase the density of the air in the room; but this will not affect the thermometer, or in a sensible degree the barometer.

There are many natural localities on the earth where exhalations affect the density of the air, but of which the presence escapes detection by the before-named instruments; and at all places we shall find, on consideration, that the density of the air depends on circumstances not necessarily shown to exist by them. If, for instance, the barometric pressure and the temperature were given, we might still have an *upper* current of wind which had a particular hygrometric state and a particular proportion of its constituent gases, whilst the lower current of wind was of another composition. The varying density of the lower current would clearly not be indicated by the usual meteorological instruments.

These cases would be comprehended in the discussion of the formula $p = kp(1 + \alpha\theta^{\circ})$ for the relation of the pressure,

* Communicated by the Author.

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density, and temperature of a gas, by the consideration that when it is applied to a mixture of gases, such as the atmosphere, the value of k is different for every variation in the proportions of the elements.

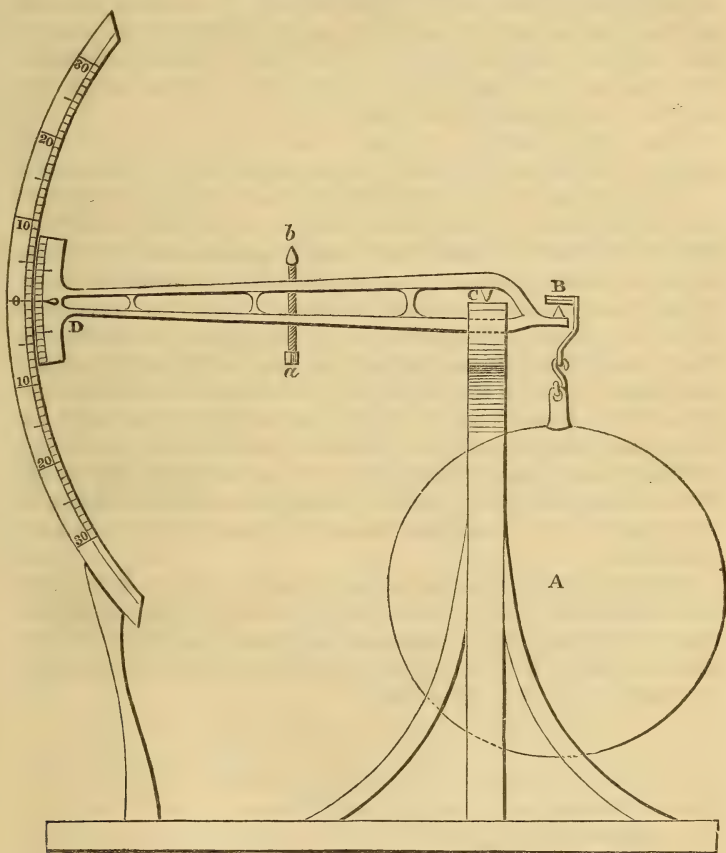
The method of weighing a known volume of the air, which has been admitted into a flask partially exhausted by the air-pump and then weighed, is one of considerable labour, and requiring the greatest degree of care in order to obtain correct results. An instrument which would give the actual density by a simple reading off is evidently so great a desideratum, that the instrument about to be described has probably in some shape or other suggested itself to others, though no instrument of the kind is used at present amongst the recognized meteorological instruments that I am aware of.

The new instrument is a modification of an old air-pump experiment, which is used to show that the greater buoyancy of more bulky over denser bodies of equal weights in the air is a considerable and a measurable quantity. A metallic body hanging at one end of a balance appears in the air to be heavier than a closed hollow globe of glass hanging from the other end; but when placed under the receiver of an air-pump, and the pump is worked, it is soon seen that as the air is withdrawn the apparent excess of weight becomes less and less, and eventually the hollow globe preponderates. In an instrument for determining the variable buoyancy of bodies in the lower stratum of the atmosphere, the points to be sought for in the construction are, a sufficient sensibility and a means of determining the results with readiness. A large closed globe of glass might be weighed at different times in a fine chemical balance; but the process would be tedious, and from perpetual changes in temperature, susceptible of less nicety than that of a delicate bent lever balance, such as I have adopted.

There arise difficulties in the construction when extreme sensibility is desired, which a knowledge of the mechanical properties of balances, and a confidence in their theory, alone can be expected to surmount. For instance, in the trials which have been made with the instrument constructed by Messrs. Watkins and Hill from my drawings, we have found a position of stable equilibrium and another of unstable equilibrium within an angle of 40 degrees. The theory showing that no such positions of equilibrium can exist in a perfect balance, the result indicated that the knife-edges of the balance, or the agate planes in contact with them, though made with all the care considered requisite in fine chemical balances, were yet not sufficiently true for such a degree of sensibility;

and that even with a less degree of sensibility it was requisite to have the knife-edges sharper, straighter, or more nearly parallel, and the agates more accurately flat;—these points were accomplished by improved methods of preparing the parts above named.

The annexed figure is a side view of the essential parts of the instrument. A is a hollow globe of glass of about $4\frac{1}{2}$



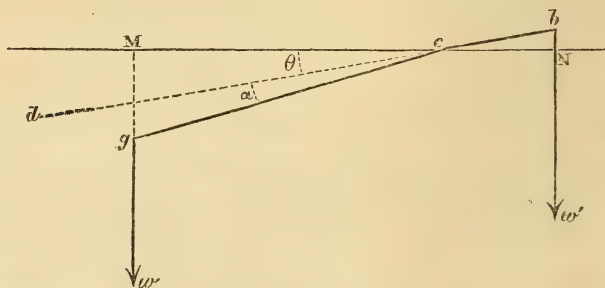
inches diameter, hermetically sealed, having an eye in the upper part where the tube from which it was blown has been sealed: by this eye it is suspended by hooks from a steel stem fixed to the brass frame B, holding an agate plate. This agate plate rests on the knife-edge of the triangular prism of steel as in the figure. C is another triangular prism of steel fixed like the former to the brass lever of the balance. The

knife-edge at C rests on another agate-plate held by the framework of the instrument. The end D of the lever is formed so as to be graduated as a vernier; and a graduated arc having its centre at the knife-edge C is fixed firmly in a vertical plane to the brass framework. The readings of the graduated arc by means of the vernier give the position of equilibrium of the lever. At *a* *b* is a fine steel screw passing through the arm of the lever and carrying a brass head at *b*; this screw, turned by a key, regulates the sensibility of the balance by raising or lowering the centre of gravity of the lever. The spirit-levels for adjustment to the horizontal position, as well as other details, are not drawn in the figure.

The whole balance is fixed firmly in a glass case or lanthorn, which also contains its accompanying thermometer.

The instrument evidently acts by the changing weight of the air displaced by the globe with its appendages, and the lever. If these volumes were exactly known, and the place of the centre of gravity of that displaced by the lever for given temperatures, then the density of the air could be calculated from the position of equilibrium; but since these cannot be easily determined with accuracy, the method of employing the instrument must be that of finding the value of its divisions in terms of the density of the air, by *experimental methods*, for changes of temperature and pressure; such as by examining the instrument under the receiver of an air-pump furnished with a barometer gauge, and by noting the effect of change of temperature in the lanthorn when the atmospheric pressure is stationary.

In investigating the theoretical sensibility of the bent lever balance, I shall first suppose, in the ordinary way, that the weights are absolute weights, and afterwards take the modification introduced by supposing the balance used, as actually, in a medium of varying density.



Let *c* be the fulcrum, *b* the point from which the globe and

its appendages hang, their weight being w' ; let g be the centre of gravity of the beam at which its weight w acts; let $cb=r'$, $cg=r$. Drawing the straight lines bcd and cg , let the angle $dcb=\alpha$; also drawing the horizontal line McN , let the angle Mcd ($=$ angle Ncb) $=\theta$.

Then in equilibrium,

$$w \times cM = w' \times cN,$$

or

$$w \times r \cos (\alpha + \theta) = w' \times r' \cos \theta,$$

$$\therefore \tan \theta = \cot \alpha \left(1 - \frac{w'.r'}{w.r.\cos \alpha} \right);$$

let

$$\frac{w'.r'}{w.r.} = 1 + x,$$

where x will be a small fraction, positive or negative; we have

$$\begin{aligned} \tan \theta &= \frac{1 - 2 \sin^2 \frac{\alpha}{2} - (1 + x)}{2 \sin \frac{\alpha}{2} \cdot \cos \frac{\alpha}{2}} \\ &= - \tan \frac{\alpha}{2} - \frac{x}{\sin \alpha}. \end{aligned}$$

Here $\theta=0$ when

$$x = -2 \sin^2 \frac{\alpha}{2};$$

also for a given value $\pm$ of x , θ changes very rapidly for corresponding changes in the value of α when this latter is very small: or the sensibility increases very quickly when the points b , c , and g are brought very nearly into one straight line.

Again, let w and w' be the apparent weights in air, and let w_1 and w_2 be the absolute weights; so that $w=w_1(1-m)$, $w'=w_2(1-n)$, where by the laws of hydrostatics mw_1 is the weight of the air displaced by the beam supposed of homogeneous materials from which it differs very slightly, and nw_2 that displaced by the globe and appendages;

$$\text{or } m = \frac{\text{specific gravity of air}}{\text{specific gravity of the beam}} = \frac{a}{b} \text{ say};$$

$$n = \frac{\text{specific gravity of air}}{\text{specific gravity of globe and appendages}} = \frac{a}{c} \text{ say}.$$

Here m and n are small quantities, but m much smaller than n , so that

$$\begin{aligned}\frac{w'.r'}{w.r} &= 1 + x \\ &= \frac{w_2.r'(1-n)}{w_1.r(1-m)} \\ &= \frac{w_2.r'}{w_1.r} (1 - \overline{n-m}) \text{ nearly} \\ &= \frac{w_2.r'}{w_1.r} \left\{ 1 - a \left(\frac{1}{c} - \frac{1}{b} \right) \right\};\end{aligned}$$

and the value of x will be continually changing as the value of a , the specific gravity of the air, changes, whilst the fraction $\frac{w_2.r'}{w_1.r}$ remains constant, and the changes in b and c are almost imperceptible in comparison with those of a : or θ , with the position of equilibrium, will continually change with the density of the atmosphere.

The degree of sensibility may be any that is wished, provided weights are added to, and taken away from the end B, which weights might be very conveniently laid upon the plate containing the agate plane. In some trials I had the index moving through 40° with $\frac{1}{2}$ grain difference in the weight hanging from B; and this is equivalent to a change of about one inch in the height of the barometer, or between 16° and 17° of Fahrenheit's thermometer.

When changes in the density of the air are required to be found which are not dependent solely on temperature and pressure, but also on occasional admixtures in the air surrounding the balance, we have to compare its indications with those of the other named instruments, and it is needless to have a greater sensibility than theirs. Now the best barometers read only to one-thousandth of an inch in the height of the mercurial column, and the best thermometers can seldom be depended upon to one-hundredth of a degree Fahrenheit; so that if $\frac{1}{2}$ a grain added at B cause the index to move through 10° of arc, and we read the vernier to $\frac{1}{2}$ minutes, we have an accuracy greater than either of the other named instruments, and one which is sufficient for our experiments. In this case the scale of 60° will suffice for the changes which occur in this climate, without need of weights in addition.

The following table contains the last series of observations which I have made previous to the instrument being finished, with some additions. The thermometer inclosed in the lanthorn was a good one, but not the one intended to be used, which was not ready; it was read with a reading microscope, as was also the ærometric balance. The instrument was placed in an upper room facing the west, in which no fire was lighted. The barometer was a good aneroid one, with an attached thermometer. I have not thought it necessary to give the readings of these, but only the height of the barometric column reduced to a temperature of 32°.

The column of the densities is calculated from the formula

$$\rho = \frac{p}{k(1 + \alpha\theta^{\circ})}$$

with unity as the density for 32° temperature and 30 inches barometric pressure, taking $\alpha = \frac{1}{490}$ for comparison with the ærometric balance. Although the changes take the same direction, that is, greater buoyancy is indicated when there was greater calculated density, yet the changes are not proportional; and on comparing different parts of the series, we see remarkable differences. Observations of the *wet bulb* thermometer might have enabled us to explain some of these, by the hygrometric changes going on; but if any remained, they would indicate changes in the composition of the air of the room.

When Messrs. Watkins and Hill have got the construction of the ærometric balance perfected, I should feel great interest in the results obtained in different parts of the country, but must leave the observations to others with better opportunities and more time, and rest satisfied with having completed the less important task of designing and superintending the constructions of the means of observation.

The observations of the table have a point of interest in having been taken through one period of storm, and another of dark but high fog.

Date. 1850.	Hour of ob- servation. :	Barome- ter cor- rected to 32° temp.	Thermo- meter in the lan- thorn.	Density of the air from $\rho = \frac{p}{k(1 + \alpha \theta^\circ)}$	Reading of the aërome- tric ba- lance.	Remarks.
April 12.	h m	in.				
	8 7 A.M.	29.55	55.5	.94257	+2 3	Rain, foggy.
	3 18 P.M.	29.72	60.9	.93870	3 35	
13.	8 5 A.M.	29.83	55.3	.95185	1 3	
	11 25	29.83	56.1	.95045	1 3	Dark, but high fog.
	11 34	29.83	56.4	.94993	1 8	Wind, lighter.
	7 40 P.M.	29.73	57.2	.94536	1 38	Dull, rainy.
	11 55	29.67	55.5	.94639	1 18	The same.
15.	7 55 A.M.	29.43	54.1	.94115	1 33	Dull, cloudy.
	3 10 P.M.	29.26	59.7	.92618	4 13	Has rained, now sunshine.
	11 37	29.22	57.0	.92948	3 29	Clear, starlight.
16.	8 5 A.M.	29.12	54.6	.93039	3 4	Wind high, with rain.
	2 0 P.M.	29.05	55.2	.92713	3 9	Storm and rain.
	3 55	29.00	55.5	.92502	3 37	High wind, sunshine.
	5 20	29.01	55.9	.92466	3 56	Very high wind.
	11 10	29.16	54.8	.93132	3 7	High wind.
17.	8 10 A.M.	29.41	53.3	.94190	2 16	Bright, strong wind.
	1 35 P.M.	29.50	58.5	.93582	3 13	Fair, sunshine, some wind.
	11 10	29.69	59.2	.94065	3 2	Starlight.
18.	8 6 A.M.	29.86	54.6	.95403	0 52	Bright sunshine.
	3 0 P.M.	30.02	63.2	.94423	3 52	The same.

IX. *Experimental Researches in Electricity*.—*Twenty-third Series.* By MICHAEL FARADAY, Esq., D.C.L., F.R.S., *Fullerian Prof. Chem. Royal Institution**.

§ 29. *On the polar or other condition of diamagnetic bodies.*

2640. **F**OUR years ago I suggested that all the phænomena presented by diamagnetic bodies, when subjected to the forces in the magnetic field, might be accounted for by assuming that they then possessed a polarity the same in kind as, but the reverse in direction of, that acquired by iron, nickel and ordinary magnetic bodies under the same circumstances (2429. 2430.). This view was received so favourably by Plücker, Reich and others, and above all by W. Weber†, that I had great hopes it would be confirmed; and though certain experiments of my own (2497.) did not increase that hope, still my desire and expectation were in that direction.

2641. Whether bismuth, copper, phosphorus, &c., when

* From the *Philosophical Transactions* for 1850, part i.; having been received by the Royal Society January 1, and read March 7 and 14, 1850.

† Poggendorff's *Annalen*, January 7, 1848; or Taylor's *Scientific Memoirs*, vol. v. p. 477.

in the magnetic field, are polar or not, is however an exceedingly important question; and very essential and great differences, in the mode of action of these bodies under the one view or the other, must be conceived to exist. I found that in every endeavour to proceed by induction of experiment from that which is known in this department of science to the unknown, so much uncertainty, hesitation and discomfort arose from the unsettled state of my mind on this point, that I determined, if possible, to arrive at some experimental proof either one way or the other. This was the more needful, because of the conclusion in the affirmative to which Weber had come in his very philosophical paper; and so important do I think it for the progress of science, that, in those imperfectly developed regions of knowledge, which form its boundaries, our conclusions and deductions should not go far beyond, or at all events not aside from the results of experiment (except as suppositions), that I do not hesitate to lay my present labours, though they arrive at a negative result, before the Royal Society.

2642. It appeared to me that many of the results which had been supposed to indicate a polar condition, were only consequences of the law that diamagnetic bodies tend to go from stronger to weaker places of action (2418.); others again appeared to have their origin in induced currents (26. 2338.); and further consideration seemed to indicate that the differences between these modes of action and that of a real polarity, whether magnetic or diamagnetic, might serve as a foundation on which to base a mode of investigation, and also to construct an apparatus that might give useful conclusions and results in respect of this inquiry. For, if the polarity exists it must be in the particles and for the time permanent, and therefore distinguishable from the momentary polarity of the mass due to induced temporary currents; and it must also be distinguishable from ordinary magnetic polarity by its contrary direction.

2643. A straight wooden lever, 2 feet in length, was fixed by an axis at one end, and by means of a crank and wheel made to vibrate in a horizontal plane, so that its free extremity passed to and fro through about two inches. Cylinders or cores of metal or other substances, $5\frac{1}{2}$ inches long and three-quarters of an inch diameter, were fixed in succession to the end of a brass rod 2 feet long, which itself was attached at the other end to the moving extremity of the lever, so that the cylinders could be moved to and fro in the direction of their length through the space of 2 inches. A large cylinder electro-magnet was also prepared (2191.), the iron core of

which was 21 inches long and 1·7 inch in diameter ; but one end of this core was made smaller for the length of 1 inch, being in that part only 1 inch in diameter.

2644. On to this reduced part was fixed a hollow helix consisting of 516 feet of fine covered copper wire: it was 3 inches long, 2 inches external diameter, and 1 inch internal diameter: when in its place, 1 inch of the central space was occupied by the reduced end of the electro-magnet core which carried it; and the magnet and helix were both placed concentric with the metal cylinder above-mentioned, and at such a distance that the latter, in its motion, would move within the helix in the direction of its axis, approaching to and receding from the electro-magnet in rapid or slow succession. The least and greatest distances of the moving cylinder from the magnet during the journey were one-eighth of an inch and 2·2 inches. The object of course was to observe any influence upon the experimental helix of fine wire which the metal cylinders might exert, either whilst moving to or from the magnet, or at different distances from it*.

2645. The extremities of the experimental helix wire were connected with a very delicate galvanometer, placed 18 or 20 feet from the machine, so as to be unaffected directly by the electro-magnet; but a commutator was interposed between them. This commutator was moved by the wooden lever (2643.), and as the electric currents which would arrive at it from the experimental helix, in a complete cycle of motion or to and fro action of the metal cylinder (2643.), would consist of two contrary portions, so the office of this commutator was, sometimes to take up these portions in succession and send them on in one consistent current to the galvanometer, and at other times to oppose them and to neutralize their result; and therefore it was made adjustable, so as to change at any period of the time or part of the motion.

2646. With such an arrangement as this, it is known that, however powerful the magnet, and however delicate the other parts of the apparatus, no effect will be produced at the galvanometer as long as the magnet does not change in force, or in its action upon neighbouring bodies, or in its distance from, or relation to, the experimental helix; but the introduction of a piece of iron into the helix, or anything else that can influence or be influenced by the magnet, can, or ought to, show a corresponding influence upon the helix and galvanometer.

* It is very probable that if the metals were made into cylinders shorter, but of larger diameter than those described above, and used with a corresponding wider helix, better results than those I have obtained would be acquired.

My apparatus I should imagine, indeed, to be almost the same in principle and practice as that of M. Weber (2640.), except that it gives me contrary results.

2647. But to obtain correct conclusions, it is most essential that extreme precaution should be taken in relation to many points which at first may seem unimportant. All parts of the apparatus should have perfect steadiness, and be fixed almost with the care due to an astronomical instrument; for any motion of any portion of it is, from the construction, sure to synchronize with the motion of the commutator; and portions of effect, inconceivably small, are then gathered up and made manifest as a whole at the galvanometer; and thus, without care, errors might be taken for real and correct results. Therefore, in my arrangements, the machine (2643, &c.), the magnet and helix, and the galvanometer stood upon separate tables, and these again upon a stone floor laid upon the earth; and the table carrying the machine was carefully strutted to neighbouring stone-work.

2648. Again, the apparatus should itself be perfectly firm and without shake in its motion, and yet easy and free. No iron should be employed in any of the moving parts. I have springs to receive and convert a portion of the momentum of the whole at the end of the to and fro journey; but it is essential that these should be of hammered brass or copper.

2649. It is absolutely necessary that the cylinder or core in its motion should not in the least degree disturb or shake the experimental helix and the magnet. Such a shake may easily take place and yet (without much experience) not be perceived. It is important to have the cores of such bodies as bismuth, phosphorus, copper, &c., as large as may be, but I have not found it safe to have less than one-eighth of an inch of space between them and the interior of the experimental helix. In order to float, as it were, the core in the air, it is convenient to suspend it in the bight or turn of a fine copper wire passing once round it, the ends of which rise up, and are made fast to two fixed points at equal heights but wide apart, so that the wire has a V form. This suspension keeps the core parallel to itself in every part of its motion.

2650. The magnet, when excited, is urged by an electric current from five pairs of Grove's plates, and is then very powerful. When the battery is not connected with it, it still remains a magnet of feeble power, and when thus employed may be referred to as in the *residual state*. If employed in the residual state, its power may for the time be considered constant, and the experimental helix may at any moment be connected with the galvanometer without any current appear-

ing there. But if the magnet be employed in the excited state, certain important precautions are necessary; for upon connecting the magnet with the battery and then connecting the experimental helix with the galvanometer, a current will appear at the latter, which will, in certain cases, continue for a minute or more, and which has the appearance of being derived at once from that of the battery. It is not so produced, however, but is due to the *time* occupied by the iron core in attaining its maximum magnetic condition (2170. 2332.), during the whole of which it continues to act upon the experimental helix, producing a current in it. This time varies with several circumstances, and in the same electro-magnet varies especially with the period during which the magnet has been out of use. When first employed, after two or three days' rest, it will amount to eighty or ninety seconds, or more. On breaking battery contact and immediately renewing it, the effect will be repeated, but occupy only twenty or thirty seconds. On a third intermission and renewal of the current, it will appear for a still shorter period; and when the magnet has been used at short intervals for some time, it seems capable of receiving its maximum power almost at once. In every experiment it is necessary to wait until the effect is shown by the galvanometer to be over; otherwise the last remains of such an effect might be mistaken for a result of polarity, or some peculiar action of the bismuth or other body under investigation.

2651. The galvanometer employed was made by Ruhmkorff and was very sensible. The needles were strengthened in their action and rendered so nearly equal, that a single vibration to the right or to the left occupied from sixteen to twenty seconds. When experimenting with such bodies as bismuth or phosphorus, the place of the needle was observed through a lens. The perfect communication in all parts of the circuit was continually ascertained by a feeble thermoelectric pair, warmed by the fingers. This was done also for every position of the commutator, where the film of oxide formed on any part by two or three days' rest was quitesufficient to intercept a feeble current.

2652. In order to bring the phænomena afforded by magnetic and diamagnetic bodies into direct relation, I have not so much noted the currents produced in the experimental helix, as the effects obtained at the galvanometer. It is to be understood, that the standard of deviation, as to direction, has always been that produced by an iron wire moving in the same direction at the experimental helix, and with the same condition of the commutator and connecting wires, as the

piece of bismuth or other body whose effects were to be observed and compared.

2653. A thin glass tube, of the given size (2643.), $5\frac{1}{2}$ by $\frac{3}{4}$ inches, was filled with a saturated solution of protosulphate of iron, and employed as the experimental core: the velocity given to the machine at this and all average times of experiment was such as to cause five or six approaches and withdrawals of the core in one second; yet the solution produced no sensible indication at the galvanometer. A piece of magnetic glass tube (2354.), and a core of foolscap paper, magnetic between the poles of the electro-magnet, were equally inefficient. A tube filled with small crystals of protosulphate of iron caused the needle to move about 2° , and cores formed out of single large crystals, or symmetric groups of crystals of sulphate of iron, produced the same effect. Red oxide of iron (colcothar) produced the least possible effect. Iron scales and metallic iron (the latter as a thin wire) produced large effects.

2654. Whenever the needle moved, it was consistent in its direction with the effect of a magnetic body; but in many cases, with known magnetic bodies, the motion was little or none. This proves that such an arrangement is by no means so good a test of magnetic polarity as the use of a simple or an astatic needle. This deficiency of power in that respect does not interfere with its ability to search into the nature of the phenomena that appear in the experiments of Weber, Reich and others.

2655. Other metals than iron were now employed and with perfect success. If they were magnetic, as nickel and cobalt, the deflection was in the same direction as for iron. When the metals were diamagnetic, the deflection was in the contrary direction; and for some of the metals, as copper, silver and gold, it amounted to 60° or 70° , which was permanently sustained as long as the machine continued to work. But the deflection was not the greatest for the most diamagnetic substances, as bismuth or antimony, or phosphorus; on the contrary, I have not been able to assure myself, up to this time, that these three bodies can produce any effect. Thus far the effect has been proportionate to the *conducting power* of the substance for electricity. Gold, silver and copper have produced large deflections, lead and tin less. Platina very little. Bismuth and antimony none.

2656. Hence there was every reason to believe that the effects were produced by the currents induced in the mass of

the moving metals, and not by any polarity of their particles. I proceeded therefore to test this idea by different conditions of the cores and the apparatus.

2657. In the first place, if produced by induced currents, the great proportion of these would exist in the part of the core near to the dominant magnet, and but little in the more distant parts; whereas in a substance like iron, the polarity which the whole assumes makes length a more important element. I therefore shortened the core of copper from $5\frac{1}{2}$ inches (2643.) to 2 inches, and found the effect not sensibly diminished; even when 1 inch long it was little less than before. On the contrary, when a fine iron wire, $5\frac{1}{2}$ inches in length, was used as core, its effects were strong; when the length was reduced to 2 inches, they were greatly diminished; and again, with a length of 1 inch, still further greatly reduced. It is not difficult to construct a core of copper, with a fine iron wire in its axis, so that when above a certain length it should produce the effects of iron, and beneath that length the effects of copper.

2658. In the next place, if the effect were produced by induced currents in the mass (2642.), division of the mass would stop these currents and so alter the effect; whereas if produced by a true diamagnetic polarity, division of the mass would not affect the polarity seriously, or in its essential nature (2430.). Some copper filings were therefore digested for a few days in dilute sulphuric acid to remove any adhering iron, then well-washed and dried, and afterwards warmed and stirred in the air, until it was seen by the orange colour that a very thin film of oxide had formed upon them: they were finally introduced into a glass tube (2653.) and employed as a core. It produced no effect whatever, but was now as inactive as bismuth.

2659. The copper may however be divided so as either to interfere with the assumed currents or not, at pleasure. Fine copper wire was cut up into lengths of $5\frac{1}{2}$ inches, and as many of these associated together as would form a compact cylinder three-quarters of an inch in diameter (2643); it produced no effect at the galvanometer. Another copper core was prepared by associating together many discs of thin copper plate, three-quarters of an inch in diameter, and this affected the galvanometer, holding its needle 25° or 30° from zero.

2660. I made a solid helix cylinder, three-quarters of an inch in diameter and 2 inches long, of covered copper wire, one-sixteenth of an inch thick, and employed this as the experimental core. When the two ends of its wire were unconnected, there was no effect upon the experimental helix, and

consequently none at the galvanometer ; but when the ends were soldered together, the needle was well affected. In the first condition, the currents, which tended to be formed in the mass of moving metal, could not exist because the metal circuit was interrupted ; in the second they could, because the circuit was not interrupted ; and such division as remained did not interfere to prevent the currents.

2661. The same results were obtained with other metals. A core cylinder of gold, made of half-sovereigns, was very powerful in its effect on the galvanometer. A cylinder of silver, made of sixpenny pieces, was very effectual ; but a cylinder made of precipitated silver, pressed into a glass tube as closely as possible, gave no indications of action whatever. The same results were obtained with disc cylinders of tin and lead, the effects being proportionate to the condition of tin and lead as bad conductors (2655.).

2662. When iron was divided, the effects were exactly the reverse in kind. It was necessary to use a much coarser galvanometer and apparatus for the purpose ; but that being done, the employment of a solid iron core, and of another of the same size or weight formed of lengths of fine iron wire (2659.), showed that the division had occasioned no inferiority in the latter. The excellent experimental researches of Dove* on the electricity of induction, will show that this ought to be the case.

2663. Hence the result of division in the diamagnetic metals is altogether of a nature to confirm the conclusion, that the effects produced by them are due to induced currents moving through their masses, and not to any polarity correspondent in its general nature (though opposed in its direction) to that of iron.

2664. In the third place (2656.), another and very important distinction in the actions of a diamagnetic metal may be experimentally established according as they may be due either to a true polarity, or merely to the presence of temporary induced currents ; and as for the consideration of this point diamagnetic and magnetic polarity are the same, the point may best be considered, at present, in relation to iron.

2665. If a core of any kind be advanced towards the dominant magnet and withdrawn from it by a motion of uniform velocity, then a complete journey or *to* and *from* action might be divided into four parts ; the *to*, the *stop* after it ; the *from*, and the *stop* succeeding that. If a core of iron make this journey, its end towards the dominant magnet becomes a pole, rising in force until at the nearest distance, and falling in force

* Taylor's Scientific Memoirs, vol. v. p. 129. I do not see a date to the paper.

until at the greatest distance. Both this effect, and its *progression* inwards and outwards, cause currents to be induced in the surrounding helix, and these currents are in one direction as the core advances, and in the contrary direction as it recedes. In reality, however, the iron does not travel with a constant velocity; for, because of the communication of motion from a revolving crank at the machine (2643.), it, in the *to* part of the journey, gradually rises from a state of rest to a maximum velocity, which is half-way, and then as gradually sinks to rest again near the magnet:—and the *from* part of the journey undergoes the same variations. Now as the maximum effect upon the surrounding experimental helix depends upon the velocity conjointly with the intensity of the magnetic force in the end of the core, it is evident that it will not occur with the maximum velocity, which is in the middle of the *to* or *from* motion; nor at the *stop* nearest to the dominant magnet, where the core end has greatest magnetic force, but somewhere between the two. Nevertheless, during the *whole* of the advance, the core will cause a current in the experimental helix in one direction, and during the whole of the recession it will cause a current in the other direction.

2666. If diamagnetic bodies, under the influence of the dominant magnet, assume also a polar state, the difference between them and iron being only that the poles of like names or forces are changed in place (2429. 2430.), then the same kind of action as that described for iron would occur with them; the only difference being, that the two currents produced would be in the reverse direction to those produced by iron.

2667. If a commutator, therefore, were to be arranged to gather up these currents, either in the one case or the other, and send them on to the galvanometer in one consistent current, it should change at the moments of the two *stops* (2665.), and then would perform such duty perfectly. If, on the other hand, the commutator should change at the times of maximum velocity or maximum intensity, or at two other times equidistant either from the one *stop* or from the other, then the parts of the opposite currents intercepted between the changes would exactly neutralize each other, and no final current would be sent on to the galvanometer.

2668. Now the action of the iron is, by experiment, of this nature. If an iron wire be simply introduced or taken out of the experimental helix with different conditions of the commutator, the results are exactly those which have been stated. If the machine be worked with an iron wire core, the commutator changing at the stops (2665.), then the current gathered

up and sent on to the galvanometer is a maximum; if the commutator change at the moments of maximum velocity, or at any other pair of moments equidistant from the one stop or the other, then the current at the commutator is a minimum, or 0.

2669. There are two or three precautions which are necessary to the production of a pure result of this kind. In the first place, the iron ought to be soft and not previously in a magnetic state. In the next, an effect of the following kind has to be guarded against. If the iron core be away from the dominant magnet at the beginning of an experiment, then, on working the machine, the galvanometer will be seen to move in one direction for a few moments, and afterwards, notwithstanding the continued action of the machine, will return and gradually take up its place at 0° . If the iron core be at its shortest distance from the dominant magnet at the beginning of the experiment, then the galvanometer needle will move in the contrary direction to that which it took before, but will again settle at 0° . These effects are due to the circumstance, that, when the iron is away from the dominant magnet, it is not in so strong a magnetic state, and when at the nearest to it is in a stronger state, than the *mean* or *average state*, which it acquires during the continuance of an experiment; and that in rising or falling to this average state, it produces two currents in contrary directions, which are made manifest in the experiments described. These existing only for the first moments, do, in their effects at the galvanometer, then appear, producing a vibration which gradually passes away.

2670. One other precaution I ought to specify. Unless the commutator changes accurately at the given points of the journey, a little effect is gathered up at each change, and may give a permanent deflection of the needle in one direction or the other. The tongues of my commutator, being at right angles to the direction of motion and somewhat flexible, dragged a little in the *to* and *from* parts of the journey: in doing this they approximated, though only in a small degree, to that which is the best condition of the commutator for gathering up (and not opposing) the currents; and a deflection to the right or left appeared (2677.). Upon discovering the cause and stiffening the tongues so as to prevent their flexure, the effect disappeared, and the iron was perfectly inactive.

2671. Such therefore are the results with an iron core, and such would be the effects with a copper or bismuth core if they acted by a diamagnetic polarity. Let us now consider what the consequences would be if a copper or bismuth

core were to act by currents, induced for the time, in its moving mass, and of the nature of those suspected (2642.). If the copper cylinder moved with uniform velocity (2665.), then currents would exist in it, parallel to its circumference, during the whole time of its motion; and these would be at their maximum force just before and just after the *to* or inner stop, for then the copper would be in the most intense parts of the magnetic field. The rising current of the copper core for the *in* portion of the journey would produce a current in one direction in the experimental helix, the stopping of the copper and consequent falling of its current would produce in the experimental helix a current contrary to the former; the first instant of motion *outwards* in the core would produce a maximum current in it contrary to its former current, and producing in the experimental helix its inductive result, being a current the same as the last there produced; and then, as the core retreated, its current would fall, and in so doing and by its final stop, would produce a fourth current in the experimental helix, in the same direction as the first.

2672. The four currents produced in the experimental helix alternate by twos, *i. e.* those produced by the falling of the first current in the core and the rising of the second and contrary current, are in one direction. They occur at the instant before and after the stop at the magnet, *i. e.* from the moment of maximum current (in the core) before, to the moment of maximum current after, the stop; and if that stop is momentary, they exist only for that moment, and should during that brief time be gathered up by the commutator. Those produced in the experimental helix during the falling of the second current in the core and the rising of a third current (identical with the first) in the return of the core to the magnet, are also the same in direction, and continue from the beginning of the retreat to the end of the advance (or from maximum to maximum) of the core currents, *i. e.* for almost the whole of the core journey; and these, by its change at the maximum moments, the commutator should take up and send on to the galvanometer.

2673. The motion however of the core is not uniform in velocity, and so sudden in its change of direction, but, as before said (2665.), is at a maximum as respects velocity in the middle of its approach to and retreat from the dominant magnet; and hence a very important advantage. For its stop may be said to commence immediately after the occurrence of the maximum velocity; and if the lines of magnetic force were equal in position and power there to what they are nearer to the magnet, the contrary currents in the experimental helix

would commence at those points of the journey; but, as the core is entering into a more intense part of the field, the current in it still rises though the velocity diminishes, and the consequence is, that the maximum current in it neither occurs at the place of greatest velocity, nor of greatest force, but at a point between the two. This is true both as regards the approach and the recession of the core, the two maxima of the currents occurring at points equidistant from the place of rest near the dominant magnet.

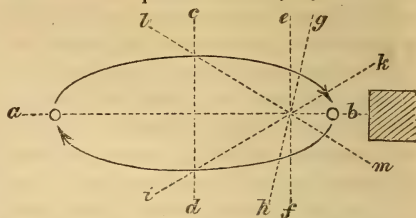
2674. It is therefore at these two points that the commutator should change, if adjusted to produce the greatest effect at the galvanometer by the currents excited in the experimental helix, through the influence of, or in connexion with, currents of induction produced in the core; and experiment fully justifies this conclusion. If the length of the journey from the stop out to the stop in, which is 2 inches (2643. 2644.), be divided into 100 parts, and the dominant magnet be supposed to be on the right-hand, then such an expression as the following, 50|50, may represent the place where the commutator changes, which in this illustration would be midway in the to and from motion, or at the places of greatest velocity.

2675. Upon trial of various adjustments of the commutator, I have found that from 77|23 to 88|12, gave the best result with a copper core. On the whole, and after many experiments, I conclude that with the given strength of electromagnet, distance of the experimental core when at the nearest from the magnet, length of the whole journey, and average velocity of the machine, 86|14 may represent the points where the induced currents in the core are at a maximum and where the commutator ought to change.

2676. From what has been said before (2667.), it will be seen that both in theory and experiment these are the points in which the effect of any polarity, magnetic or diamagnetic, would be absolutely nothing. Hence the power of submitting by this machine metals and other bodies to experiment, and of eliminating the effects of magnetic polarity, of diamagnetic polarity, and of inductive action, the one from the others: for either by the commutator or by the direction of the polarity, they can be separated; and further, they can also be combined in various ways for the purpose of elucidating their joint and separate action.

2677. For let the arrows in the diagram represent the to and from journey, and the intersections of the lines *a*, *b* or *c*, *d*, &c. the periods in the journey when the commutator changes (in which case *c*, *d* will correspond to 50|50, and *e*, *f* to 86|14), then *a*, *b* will represent the condition of the commutator for

the maximum effect of iron or any other polar body. If the line a, b be gradually revolved until parallel to c, d , it will in every position indicate points of commutator change, which will give the iron effect at the galvanometer by a deflection of the needle always in the same direction; it is only when the ends



a and b have passed the points c and d , either above or below, that the direction of the deflection will change for iron. But the line a, b indicates those points for the commutator with which no effect will be produced on the galvanometer by the induction of currents in the mass of the core. If the line be inclined in one direction, as i, k , then these currents will produce a deflection at the galvanometer on one side; if it be inclined in the other direction, as l, m , then the deflection will be on the other side. Therefore the effects of these induced currents may be either combined with, or opposed to, the effects of a polarity, whether it be magnetic or diamagnetic.

2678. All the metals before mentioned (2655.), namely, gold, silver, copper, tin, lead, platina, antimony and bismuth, were submitted to the power of the electro-magnet under the best adjustment (2675.) of the commutator. The effects were stronger than before, being now at a maximum, but in the same order; as regarded antimony and bismuth, they were very small, amounting to not more than half a degree, and may very probably have been due to a remainder of irregular action in some part of the apparatus. All the experiments with the divided cores (2658, &c.) were repeated with the same results as before. Phosphorus, sulphur and gutta percha did not, either in this or in the former state of the commutator, give any indication of effect at the galvanometer.

2679. As an illustration of the manner in which this position of the commutator caused a separation of the effects of copper and iron, I had prepared a copper cylinder core 2 inches in length having an iron wire in its axis, and this being employed in the apparatus gave the pure effect of the copper with its induced currents. Yet this core, as a whole, was highly magnetic to an ordinary test-needle; and when the two changes of the commutator were not equidistant from the one stop or the other (2670. 2677.), the iron effect came out powerfully, overruling the former and producing very strong contrary deflections at the needle. The platinum core which I have used is an imperfect cylinder, 2 inches long and 0.62 of an inch thick: it points magnetically between the poles of

a horseshoe electro-magnet (2381.), making a vibration in less than a second, but with the above condition of the commutator (2675.) gives 4° of deflection due to the induced currents, the magnetic effect being annulled or thrown out.

2680. Some of the combined effects produced by oblique position of the commutator points were worked out in confirmation of the former conclusions (2677.). When the commutator was so adjusted as to combine any polar power which the bismuth, as a diamagnetic body, might possess, with any conducting power which would permit the formation of currents by induction in its mass (2676.), still the effects were so minute and uncertain as to oblige me to say that, experimentally, it is without either polar or inductive action.

2681. There is another distinction which may usefully be established between the effects of a true sustainable polarity, either magnetic or diamagnetic, and those of the transient induced currents dependent upon *time*. If we consider the resistance in the circuit, which includes the experimental helix and the galvanometer coil, as nothing, then a magnetic pole of constant strength passed a certain distance into the helix, would produce the same amount of current electricity in it, whether the pole were moved into its place by a quick or a slow motion. Or if the iron core be used (2668.) the same result is produced, provided, in any alternating action, the core is left long enough at the extremities of its journey to acquire, either in its quick or slow alternation, the same state. This I found to be the fact when no commutator nor dominant magnet was used; a single insertion of a weak magnetic pole gave the same deflection, whether introduced quickly or slowly; and when the residual dominant magnet, an iron wire core, and the commutator in its position *a, b* (2677.) were used, four journeys to and from produced the *same* effect at the galvanometer when the velocities were as 1 : 5 or even as 1 : 10.

2682. When a copper, silver, or gold core is employed in place of the iron, the effect is very different. There is no reason to doubt that, as regards the core itself, the same amount of electricity is thrown into the form of induced circulating currents within it, by a journey to or from, whether that journey is performed quickly or slowly: the above experiment (2681.) in fact confirms such a conclusion. But the effect which is produced upon the experimental helix is not proportionate to the whole amount of these currents, but to the maximum intensities to which they rise. When the core moves slowly, this intensity is small; when it moves rapidly, it is great, and necessarily so, for the same current of elec-

tricity has to travel in the two differing periods of time occupied by the journeys. Hence the quickly moving core should produce a far higher effect on the experimental helix than the slowly moving core ; and this also I found to be the fact.

2683. The short copper core was adjusted to the apparatus, and the machine worked with its average velocity until forty journeys to and from had been completed ; the galvanometer needle passed 39° west. Then the machine was worked with a greater rapidity, also for forty journeys, and the needle passed through 80° or more west ; finally, being worked at a slow rate for the same number of journeys, the needle went through only 21° west. The extreme velocities in this experiment were probably as 1 : 6 ; the time in the longest case was considerably less than that of one vibration of the needle (2651.), so that I believe all the force in the slowest case was collected. The needle is very little influenced by the swing or momentum of its parts, because of the deadening effect of the copper plate beneath it, and, except to return to zero, moves very little after the motion of the apparatus ceases. A silver core produced the same results.

2684. These effects of induced currents have a relation to the phænomena of revulsion which I formerly described (2310. 2315. 2338.), being the same in their exciting cause and principles of action, and so the two sets of phænomena confirm and illustrate each other. That the revulsive phænomena are produced by induced currents, has been shown before (2327. 2329. 2336. 2339.) ; the only difference is, that with them the induced currents were produced by exalting the force of a magnet placed at a fixed distance from the affected metal ; whilst in the present phænomena, the force of the magnet does not change, but its distance from the piece of metal does.

2685. So also the same circumstances which affect the phænomena here affect the revulsive phænomena. A plate of metal will, as a whole, be well-revulsed ; but if it be divided across the course of the induced currents it is not then affected (2529.). A ring helix of copper wire, if the extremities be unconnected, will not exhibit the phænomena, but if they be connected then it presents them (2660.).

2686. On the whole, the revulsive phænomena are a far better test and indication of these currents than the present effects ; especially if advantage be taken of the division of the mass into plates, so as to be analogous, or rather superior, in their action to the disc cylinder cores (2659. 2661.). Platinum, palladium and lead in leaf or foil, if cut or folded into squares half an inch in the side, and then packed regularly

together, will show the phænomena of revulsion very well; and that according to the direction of the leaves, and not of the external form. Gold, silver, tin and copper have the revulsive effects thus greatly exalted. Antimony, as I have already shown, exhibits the effect well (2514. 2519.). Both it and bismuth can be made to give evidence of the induced currents produced in them when they are used in thin plates, either single or associated, although, to avoid the influence of the diamagnetic force, a little attention is required to the moments of making and breaking contact between the voltaic battery and the electro-magnet.

2687. Copper, when thus divided into plates, had its revulsive phænomena raised to a degree that I had not before observed. A piece of copper foil was annealed and tarnished by heat, and then folded up into a small square block, half an inch in the side and a quarter of an inch thick, containing seventy-two folds of the metal. This block was suspended by a silk film as before (2248.), and whilst at an angle of 30° or thereabouts with the equatorial line (2252.), the electro-magnet was excited; it immediately advanced or turned until the angle was about 45° or 50° , and then stood still. Upon the interruption of the electric current at the magnet the revulsion came on very strongly, and the block turned back again, passed the equatorial line, and proceeded on until it formed an angle of 50° or 60° on the other side; but instead of continuing to revolve in that direction as before (2315.), it then returned on its course, again passed the equatorial line, and almost reached the axial position before it stood still. In fact, as a mass, it vibrated to and fro about the equatorial line.

2688. This however is a simple result of the principles of action formerly developed (2329. 2336.). The revulsion is due to the production of induced currents in the suspended mass during the falling of the magnetism of the electro-magnet; and the effect of the action is to bring the axis of these induced currents parallel to the axis of force in the magnetic field. Consequently, if the time of the fall of magnetic force, and therefore of the currents dependent thereon, be greater than the time occupied by the revulsion of the copper block as far as the equatorial line, any further motion of it by momentum will be counteracted by a contrary force; and if this force be strong enough the block will return. The conducting power of the copper and its division into laminæ, tend to set up these currents very readily and with extra power; and the very power which they possess tends to make the time of a vibration so short, that two or even three vibrations can occur before the force of the electro-magnet has

ceased to fall any further. The effect of *time*, both in the rising and falling of power, has been referred to on many former occasions (2170. 2650.), and is very beautifully seen here.

2689. Returning to the subject of the assumed polarity of bismuth, I may and ought to refer to an experiment made by Reich, and described by Weber*, which, if I understand the instruction aright, is as follows: a strong horseshoe magnet is laid upon a table in such a position that the line joining its two poles is perpendicular to the magnetic meridian and to be considered as prolonged on one side; in that line, and near the magnet, is to be placed a small powerful magnetic needle, suspended by cocoon silk, and on the other side of it, the pole of a bar magnet, in such a position and so near, as exactly to counteract the effect of the horseshoe magnet, and leave the needle to point exactly as if both magnets were away. Then a mass of bismuth being placed between the poles of the horseshoe magnet is said to react upon the small magnet needle, causing its deflection in a particular direction, and this is supposed to indicate the polarity of the bismuth under the circumstances, as it has no such action when the magnets are away. A piece of iron in place of the bismuth produces the contrary deflection of the needle.

2690. I have repeated this experiment most anxiously and carefully, but have never obtained the slightest trace of action with the bismuth. I have obtained action with the iron; but in those cases the action was far less than if the iron were applied outside between the horseshoe magnet and the needle, or to the needle alone, the magnets being entirely away. On using a garnet, or a weak magnetic substance of any kind, I cannot find that the arrangement is at all comparable for readiness of indication or delicacy, with the use of a common or an astatic needle, and therefore I do not understand how it could become a test of the polarity of bismuth when these fail to show it. Still I may have made some mistake; but neither by close reference to the description, nor to the principles of polar action, can I discover where.

2691. There is an experiment which Plücker described to me, and which at first seems to indicate strongly the polarity of bismuth. If a bar of bismuth (or phosphorus) be suspended horizontally between the poles of the electro-magnet, it will go to the equatorial position with a certain force, passing, as I have said, from stronger to weaker places of action (2267.).

* Taylor's Scientific Memoirs, vol. v. p. 480.

If a bar of iron of the same size be fixed in the equatorial position a little below the plane in which the diamagnetic bar is moving, the latter will proceed to the equatorial position with much greater force than before, and this is considered as due to the circumstance, that, on the side where the iron has N polarity, the diamagnetic body has S polarity, and that on the other side the S polarity of the iron and the N polarity of the bismuth also coincide.

2692. It is however very evident that the lines of magnetic force have been altered sufficiently in their intensity of direction, by the presence of the iron, to account fully for the increased effect. For, consider the bar as just leaving the axial position and going to the equatorial position; at the moment of starting its extremities are in places of stronger magnetic force than before, for it cannot be doubted for a moment that the iron bar determines more force from pole to pole of the electro-magnet than if it were away. On the other hand, when it has attained the equatorial position, the extremities are under a much weaker magnetic force than they were subject to in the *same places* before; for the iron bar determines downwards upon itself much of that force, which, when it is not there, exists in the plane occupied by the bismuth. Hence, in passing through 90° , the diamagnetic is urged by a much greater difference of intensity of force when the iron is present than when it is away; and hence, probably, the whole additional result. The effect is like many others which I have referred to in magnecrystallic action (2487-2497.), and does not, I think, add anything to the experimental proof of diamagnetic polarity.

2693. Finally, I am obliged to say that I can find no experimental evidence to support the hypothetical view of diamagnetic polarity (2640.), either in my own experiments, or in the repetition of those of Weber, Reich, or others. I do not say that such a polarity does not exist; and I should think it possible that Weber, by far more delicate apparatus than mine, had obtained a trace of it, were it not that then also he would have certainly met with the far more powerful effects produced by copper, gold, silver, and the better conducting diamagnetics. If bismuth should be found to give any effect, it must be checked and distinguished by reference to the position of the commutator, division of the mass by pulverization, influence of time, &c. It appears to me also, that, as the magnetic polarity conferred by iron or nickel in very small quantity, and in unfavourable states, is far more readily indicated by its effect on an astatic needle, or by pointing between the poles of a strong horseshoe magnet, than by any such

arrangement as mine or Weber's or Reich's, so diamagnetic polarity would be much more easily distinguished in the same way, and that no indication of that polarity has as yet reached to the force and value of those already given by Brugmann and myself.

2694. So, at present, the actions represented or typified by iron, by copper and by bismuth, remain distinct; and their relations are only in part made known to us. It cannot be doubted that a larger and simpler law of action than any we are yet acquainted with, will hereafter be discovered, which shall include all these actions at once; and the beauty of Weber's suggestion in this respect was the chief inducement to me to endeavour to establish it.

2695. Though from the considerations above expressed (2693.) I had little hopes of any useful results, yet I thought it right to submit certain magnecrystallic cores to the action of the apparatus. One core was a large group of symmetrically disposed crystals of bismuth (2457.); another a very large crystal of red ferroprussiate of potassa; a third a crystal of calcareous spar; and a fourth and fifth large crystals of protosulphate of iron. These were formed into cylinders, of which the first and fourth had the magnecrystallic axes (2479.) parallel to the axis of the cylinder, and the second, third and fifth, had the equatorial direction of force (2594. 2595. 2546.) parallel to the axis of the cylinder. None of them gave any effect at the galvanometer, except the fourth and fifth, and these were alike in their results, and were dependent for them on their ordinary magnetic property.

2696. Some of the expressions I have used may seem to imply, that, when employing the copper and other cores, I imagine that currents are first induced in them by the dominant magnet, and that these induce the currents which are observed in the experimental helix. Whether the cores act directly on the experimental helix or indirectly through their influence on the dominant magnet, is a very interesting question, and I have found it difficult to select expressions, though I wished to do so, which should not in some degree prejudice that question. It seems to me probable, that the cores act indirectly on the helix, and that their immediate action is altogether directed towards the dominant magnet, which, whether they consist of magnetic or diamagnetic metals, raises them into power either permanently or transiently, and has their power for that time directed towards it. Before the core moves to approach the magnet, the magnet and experimental helix are in close relation; and the latter is situated in the intense field of magnetic force which belongs to the

pole of the former. If the core be iron, as it approaches the magnet it causes a strong convergence and concentration of the lines of magnetic force upon itself; and these, as they so converge, passing through the helix and across its convolutions, are competent to produce the currents in it which are obtained (2653. 2668.). As the iron retreats these lines of force diverge, and again crossing the line of the wire in the helix in a contrary direction to their former course, produce a contrary current. It does not seem necessary, in viewing the action of the iron core, to suppose any direct action of it on the helix, or any other action than this which it exerts upon the lines of force of the magnet. In such a case its action upon the helix would be indirect.

2697. Then, by all parity of reasoning, when a copper core enters the helix its action upon it should be indirect also. For the currents which are produced in it are caused by the direct influence of the magnet, and must react equivalently upon it. This they do, and because of their direction and known action, they will cause the lines of force of the magnet to diverge. As the core diminishes in its velocity of motion, or comes to rest, the currents in it will cease, and then the lines of force will converge; and this divergence and convergence, or passage in two directions across the wire of the experimental helix, is sufficient to produce the two currents which are obtained in the advance of the core towards the dominant magnet (2671. 2673.). A corresponding effect in the contrary direction is produced by the retreat of the core.

2698. On the idea that the actions of the core were not of this kind, but more directly upon the helix, I interposed substances between the core and the helix during the times of the experiment. A thick copper cylinder 2.2 inches long, 0.7 of an inch external diameter, and 0.1 of an inch internal diameter, and consequently 0.3 of an inch thick in the sides, was placed in the experimental helix, and an iron wire core (2668.) used in the apparatus. Still, whatever the form of the experiment, the kind and amount of effect produced was the same as if the copper were away, and either glass or air in its place. When the dominant magnet was removed and the wire core made a magnet, the same results were produced.

2699. Another copper lining, being a cylinder 2.5 inches long, 1 inch in external diameter, and one-eighth of an inch in thickness, was placed in the experimental helix, and cores of silver and copper five-eighths of an inch in thickness, employed as before, with the best condition of the commutator (2675.): the effects, with and without the copper, or with and without the glass, were absolutely the same (2698.).

2700. There can be no doubt that the copper linings, when in place, were full of currents at the time of action, and that when away no such currents would exist in the air or glass replacing them. There is also full reason to admit, that the divergence and convergence of the magnetic lines of force supposed above (2697.) would satisfactorily account for such currents in them, supposing the indirect action of the cores were assumed. If that supposition be rejected, then it seems to me that the whole of the bodies present, the magnet, the helix, the core, the copper lining, or the air or glass which replaces it, must all be in a state of tension, each part acting on every other part, being in what I have occasionally elsewhere imagined as the electro-tonic state (1729.).

2701. The advance of the copper makes the lines of magnetic force diverge, or, so to say, drives them before it (2697.). No doubt there is reaction upon the advancing copper, and the production of currents in it in such a direction as makes them competent, if continued, to continue the divergence. But it does not seem logical to say, that the currents which the lines of force cause in the copper, are the cause of the divergence of the lines of force. It seems to me, rather, that the lines of force are, so to say, diverged, or bent outward by the advancing copper (or by a connected wire moving across lines of force in any other form of the experiments), and that the reaction of the lines of force upon the forces in the particles of the copper causes them to be resolved into a current, by which the resistance is discharged and removed, and the line of force returns to its place. I attach no other meaning to the words *line of force* than that I have given on a former occasion (2149.).

Royal Institution, Dec. 14, 1849.

X. *On the Geometrical Interpretation of Quaternions.*

By WILLIAM SPOTTISWOODE, M.A., of Balliol College, Oxford*.

§ 1. *Fundamental Laws.*

THE following investigations refer to the same subject as that treated by Professor Donkin in vol. xxxvi. of this Journal; and are offered, not as at all preferable to his, but simply as indicating another mode in which the question may be viewed; it being desirable to exhibit a subject, which is somewhat new, in more than one way, in order that as much light as possible may be thrown upon it. The present paper will be interesting (if it is so at all) principally because its results are substantially the same as those of the paper just referred to,

* Communicated by the Author.

although obtained by an entirely independent process. I have stated my views as briefly as possible, because Prof. Donkin's paper renders any more lengthened discussion superfluous; and if any expressions occur which appear to indicate a view of algebraic symbols, &c. different from his, they have been used merely because they are the ordinary terms; and I should wish them to be understood as far as possible in his way, if for no better reason, at least in order that the two methods may be compared.

The calculus of quaternions is a generalization of algebra, in which sets of four ordinary algebraical quantities are used instead of single quantities. Each such set of four quantities is called a quaternion; the nature and laws of combination of which are the object of the present investigations. The corresponding laws in ordinary algebra will be assumed as known.

Let a quaternion be defined to be a set of four algebraic quantities considered with reference to their order of position, and let it be expressed by the following equation,

$$Q = (w, x, y, z), \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

in which Q , or its equivalent, is called the quaternion, and w, x, y, z its constituents. As this definition involves no law of connexion between the constituents, it is clear that the equivalence of any number of quaternions must involve the equivalence of their several constituents; so that the equations

$$Q = Q_1 = Q_2 = \dots \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

involve the following,

$$\left. \begin{array}{l} w = w_1 = w_2 = \dots \\ x = x_1 = x_2 = \dots \\ y = y_1 = y_2 = \dots \\ z = z_1 = z_2 = \dots \end{array} \right\} \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

and conversely (3.) will involve (2.). The same principle gives rise to the following law for the addition and subtraction of quaternions:

$$\Sigma Q'_n = (\Sigma w_n, \Sigma x_n, \Sigma y_n, \Sigma z_n); \quad . \quad . \quad . \quad (4.)$$

particular cases of which are

$$nQ = (nw, nx, ny, nz) \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

$$Q - Q = 0 = (0, 0, 0, 0). \quad . \quad . \quad . \quad . \quad (6.)$$

The following consideration will assist further investigations. The quaternion

$$(w, 0, 0, 0) \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

is a system consisting of the quantity w , followed by no other

quantities, *i. e.* associated with nothing but itself; in other words, it is simply equivalent to the ordinary algebraic quantity w ; so that by means of the law of addition of quaternions, it will be allowable to write,

$$\begin{aligned} (w, x, y, z) &= (w, 0, 0, 0) + (0, x, 0, 0) + (0, 0, y, 0) + (0, 0, 0, z) \\ &= w + (0, x, 0, 0) + (0, 0, y, 0) + (0, 0, 0, z) \end{aligned} \quad (8.)$$

With respect to the last three terms of this expression, it will be necessary to introduce some new symbols. Thus, for instance, if T, T', T'' indicate the operations of transposition defined by the following equations,

$$\left. \begin{aligned} T(x, 0, 0, 0) &= (0, x, 0, 0) \\ T'(y, 0, 0, 0) &= (0, 0, y, 0) \\ T''(z, 0, 0, 0) &= (0, 0, 0, z) \end{aligned} \right\}, \quad . \quad . \quad . \quad (9.)$$

the equation (8.) might be written

$$Q = w + Tx + T'y + T''z. \quad . \quad . \quad . \quad (10.)$$

And, if the laws of the combination of the symbols T, T', T'' were known, the general laws of the combination of quaternions would be at once deducible.

It will however be more advantageous to use some symbols of transposition rather different from those above noticed; let then

$$\left. \begin{aligned} iQ &= (-x, w, -z, y) \\ jQ &= (-y, z, w, -x) \\ kQ &= (-z, -y, x, w) \end{aligned} \right\}; \quad . \quad . \quad . \quad (11.)$$

from these definitions of the symbols of transposition, i, j, k , it is easy to deduce the following relations:

$$\left. \begin{aligned} i.iQ &= j.jQ = k.kQ = i.j.kQ = (-w, -x, -y, -z) = -Q \\ j.kQ &= -k.jQ = iQ \\ k.iQ &= -i.kQ = jQ \\ i.jQ &= -j.iQ = kQ \end{aligned} \right\} \quad (12.)$$

in which the expression for $-Q$ may be deduced from (5.) by writing -1 for n . These relations may be symbolically written, as follows:

$$\left. \begin{aligned} i^2 &= j^2 = k^2 = ijk = -1 \\ jk &= -kj = i \\ ki &= -ik = j \\ ij &= -ji = k \end{aligned} \right\}, \quad . \quad . \quad . \quad (13.)$$

the operations of transposition and change of sign being independent of the subject of operation.

As it will assist the geometrical interpretation of the operations i, j, k hereafter, to separate each of them into two distinct operations, the formulæ to which such separation gives rise may be properly noticed here. Let then

$$\left. \begin{aligned} i'Q &= (-x, w, y, z) & i''Q &= (w, x, -z, y) \\ j'Q &= (-y, x, w, z) & j''Q &= (w, z, y, -x) \\ k'Q &= (-z, x, y, w) & k''Q &= (w, -y, x, z) \end{aligned} \right\}; \quad (14.)$$

there will then result

$$\left. \begin{aligned} i &= i' i'' = i'' i \\ j &= j' j'' = j'' j' \\ k &= k' k'' = k'' k' \end{aligned} \right\}, \quad (15.)$$

to which may be added,

$$\left. \begin{aligned} i''j' &= k' i'', & j''k' &= i' j'', & k''i' &= j' k'', \\ j''k'' &= k'' i'' = i'' j'' \\ P(i' i' i'') &= P(j' j' j'') = P(k' k' k'') = -1 \end{aligned} \right\}, \quad . . . (16.)$$

where P represents the symbolical product without reference to order. By means of the above properties of i, j, k , it will be possible to transform the expression of a quaternion (1.) into another of the same form as (10.); for

$$\left. \begin{aligned} (0, x, 0, 0) &= i(x, 0, 0, 0) = ix \\ (0, 0, y, 0) &= j(y, 0, 0, 0) = jy \\ (0, 0, 0, z) &= k(z, 0, 0, 0) = kz \end{aligned} \right\}; \quad . . (17.)$$

so that (8.) may be written thus,

$$Q = w + ix + jy + kz, \quad (18.)$$

in which i, j, k may be combined according to the laws defined by (13.). It may be observed, that, since by means of the condition (4.) the addition of quaternions is reduced to the addition of ordinary algebraical quantities, the order and clustering of the terms in (18.) is indifferent, so that the *associative principle of addition* among those terms is completely established; the same is obviously the case with respect to the addition of quaternions in general. It may be further remarked, that, since by means of (4.),

$$\left. \begin{aligned} i\Sigma Q_n &= \Sigma iQ_n, & j\Sigma Q_n &= \Sigma jQ_n, & k\Sigma Q_n &= \Sigma kQ_n \\ jk\Sigma Q_n &= \Sigma jkQ_n, & ki\Sigma Q_n &= \Sigma kiQ_n, & ij\Sigma Q_n &= \Sigma ijQ_n \\ &= j\Sigma kQ_n & &= k\Sigma iQ_n, & &= i\Sigma jQ_n \end{aligned} \right\}, \quad (19.)$$

with other like formulæ, the distributive character of the symbols i, j, k is also established.

The following verifications, although not essential to the theory, are perhaps worth noticing. If we had taken (18.) as the definition of a quaternion with (13.) as the definitions of i, j, k , we should have found

$$\left. \begin{aligned} iQ &= iw - x + ky - jz \\ jQ &= jw - kx - y + iz \\ kQ &= kw + jx - iy - z \end{aligned} \right\}; \quad . \quad . \quad . \quad (20.)$$

so that the equations

$$Q = Q_1 = Q_2 = \dots,$$

which obviously involve also

$$\left. \begin{aligned} iQ &= iQ_1 = iQ_2 = \dots \\ jQ &= jQ_1 = jQ_2 = \dots \\ kQ &= kQ_1 = kQ_2 = \dots \end{aligned} \right\}, \quad . \quad . \quad . \quad (21.)$$

give rise to the equations (3.); for i, j, k being symbolical expressions for $(-)^{\frac{1}{2}}$, render all real terms, to which they are prefixed, imaginary in the ordinary sense of that word. The same definition of a quaternion gives

$$\Sigma Q_n = \Sigma w_n + i \Sigma x_n + j \Sigma y_n + k \Sigma z_n, \quad . \quad . \quad . \quad (22.)$$

which is in fact identical with (4.).

But the principal advantage of the linear form of the expression for a quaternion is found in the processes of multiplication and division. In the form (1.) it does not seem possible to obtain a complete solution of the problem of multiplication; the following however would be the initial steps to such a solution:

$$\begin{aligned} Q.Q_1 &= (w, x, y, z). (w_1, x_1, y_1, z_1) \\ &= \{w(w_1, x_1, y_1, z_1), x(w_1, x_1, y_1, z_1), y(w_1, x_1, y_1, z_1), z(w_1, x_1, y_1, z_1)\} \\ &= \{w(w_1, x_1, y_1, z_1), 0, 0, 0\} \\ &+ \{0, x(w_1, x_1, y_1, z_1), 0, 0\} \\ &+ \{0, 0, y(w_1, x_1, y_1, z_1), 0\} \\ &+ \{0, 0, 0, z(w_1, x_1, y_1, z_1)\} \\ &= (w_1 w, w_1 x, w_1 y, w_1 z) \\ &+ \{0, xw_1 + x(0, x_1, 0, 0) + x(0, 0, y_1, 0) + x(0, 0, 0, z_1), 0, 0\} \\ &+ \{0, 0, yw_1 + y(0, x_1, 0, 0) + y(0, 0, y_1, 0) + y(0, 0, 0, z_1), 0\} \\ &+ \{0, 0, 0, zw_1 + z(0, x_1, 0, 0) + z(0, 0, y_1, 0) + z(0, 0, 0, z_1)\} \end{aligned}$$

$$\begin{aligned}
 &= (w w_1, \quad w_1 x + x_1 w, \quad w_1 y + y_1 w, \quad w_1 z + z_1 w) \\
 &\quad + (0, x, y, z) \cdot (0, x_1, 0, 0) \\
 &\quad + (0, x, y, z) \cdot (0, 0, y_1, 0) \\
 &\quad + (0, x, y, z) \cdot (0, 0, 0, z_1)
 \end{aligned}$$

$$= (w w_1, w_1 x + x_1 w, w_1 y + y_1 w, w_1 z + z_1 w) + (0, x, y, z) \cdot (0, x_1, y_1, z_1).$$

But if we adopt the form (18.), the constituents of the product of two quaternions are completely determined; in fact, it is found without difficulty that if

$$Q Q_1 = Q_2 = w_2 + i x_2 + j y_2 + k z_2, \quad . \quad . \quad . \quad (24.)$$

$$\left. \begin{aligned}
 w_2 &= w w_1 - x x_1 - y y_1 - z z_1 \\
 x_2 &= w x_1 + w_1 x + y z_1 - y_1 z \\
 y_2 &= w y_1 + w_1 y + z x_1 - z_1 x \\
 z_2 &= w z_1 + w_1 z + x y_1 - x_1 y
 \end{aligned} \right\} . \quad . \quad . \quad . \quad (25.)$$

And also, if

$$Q_1 Q = Q_2^1 = w_2^1 + i x_2^1 + j y_2^1 + k z_2^1, \quad . \quad . \quad . \quad (26.)$$

$$w_2 = w_2^1, \quad x_2 = -x_2^1, \quad y_2 = -y_2^1, \quad z_2 = -z_2^1. \quad . \quad (27.)$$

Moreover

$$(i x + j y + k z)^2 = -x^2 - y^2 - z^2 \quad . \quad . \quad . \quad (28.)$$

$$Q^2 = w^2 - x^2 - y^2 - z^2 + 2w(i x + j y + k z) \quad . \quad (29.)$$

$$\left. \begin{aligned}
 (w - i x - j y - k z)(w + i x + j y + k z) &= w^2 + x^2 + y^2 + z^2 \\
 &= (w + i x + j y + k z)(w - i x - j y - k z).
 \end{aligned} \right\} \quad (30.)$$

So that the reciprocal of a quaternion is the quotient of the quaternion itself, with the signs of its last three constituents changed divided by the sum of the squares of the constituents.

The constituents of the ratio

$$Q_1 = Q^{-1} Q_2$$

may be found either by solving (25.) with respect to w_1, x_1, y_1, z_1 , or by means of the relation (30.), and so reducing the division to multiplication.

§ 2. Geometrical Interpretation.

In the general expression

$$Q = (w, x, y, z) = w + i x + j y + k z, \quad . \quad . \quad . \quad (1.)$$

let w, x, y, z represent straight lines drawn in several directions from the origin, and let x, y, z coincide with the three positive axes of coordinates respectively, while the direction of w is arbitrary; x, y, z may then be considered as the co-ordinates of some point, in general not the extremity of w .

In accordance with the fundamental idea of a quaternion, the position of the line represented by any constituent will be supposed to depend upon the position which that constituent holds in the first expression for Q ; so that the directions of the four lines being once chosen, the first, second, third, and fourth constituents will always represent lines drawn in the four directions respectively, whatever changes may have taken place in the order of the constituents as originally given. Now, returning to the equations (13.) and (14.) of the former section, it appears that i'' indicates a change by which the negative axis of z is brought into the old position of the axes of y ; and the axis of y into the old position of the axis of z ; the positions of w and x remaining unchanged; or i'' may be considered as indicating a change by which w and x are brought into positions such, that they are situated, with respect to the negative axis of z and the positive axis of y , in the same manner as they were at first with respect to the axes of y and z respectively. When the axes are rectangular, as at present, this change may be represented, either by supposing the plane of yz to revolve in its own plane through half a right angle in the direction from y to z , or by supposing the three axes to remain fixed, and the radius vector w to revolve on the surface of a right cone with a circular base, whose axis is that of x and vertex the origin, through one quarter of a revolution; the direction of rotation being from the axis of z towards that of y . It is easily seen that j'' and k'' may be represented by similar revolutions about the axes of y and z respectively.

Again, i' indicates a change by which the negative axis of x is brought into the old position of w , and w into the old position of the positive axis of x , the positions of y and z remaining unchanged; and if α be the angle between the axis of x and the line w (the vertical angle of the first cone), this change may be represented by bringing the negative axis of the cone into the old position of w , and then opening the vertical angle of the cone through an angle $=\pi-2\alpha$. The changes j' and k' may similarly be represented by supposing the negative axes of the other two cones respectively to take the position of w , and the vertical angle of the cones to be opened through angles $=\pi-2\beta$ and $\pi-2\gamma$ respectively. The above theory becomes much simpler when the position of w is not absolutely determined, but merely restricted to a given plane; in this case its position may be supposed to coincide with the intersection of that plane with one of the coordinate planes; *e. g.* in the case of i , with the intersection with the plane of yz ; in that of j , with that of zx ; and in that of k , with that of xy ; the three cones then become simply the three co-

ordinate planes, and i'', j'', k'' will represent rotations of this line of intersection through angles $= \frac{\pi}{2}$ in the planes of yz, zx, xy respectively; and i', j', k' similar rotations in the planes of wx, wy, wz . In each case the origin of the rotations is determinate.

If the position of w be entirely arbitrary, the positions of the intersections of the planes of wx, wy, wz with those of yz, zx, xy , will be so also; and the only difference arising in the significations of $i', j', k', i'', j'', k''$, will be that the origin of rotation is restricted only to the three coordinate planes successively, the position in those planes being arbitrary. These considerations will enable us to interpret the various terms in the linear expression for Q ; for

$$ix = i(x, 0, 0, 0) = (0, x, 0, 0) \quad . \quad . \quad . \quad (2.)$$

$$jy = j(y, 0, 0, 0) = (0, 0, y, 0) \quad . \quad . \quad . \quad (3.)$$

$$kz = k(z, 0, 0, 0) = (0, 0, 0, z) \quad . \quad . \quad . \quad (4.)$$

Now the first constituent of the quaternions on the right-hand side of the above expression will, according to the principles of interpretation above given, be considered as representing a line coinciding with the intersection of a plane passing through the axis of

x , with the plane of yz , in (2.);

y , ... zx , ... (3.);

z , ... xy , ... (4.);

and consequently ix, jy, kz will represent that the lines whose lengths are represented by x, y, z have revolved through angles each $= \frac{\pi}{2}$ in planes perpendicular to their original directions.

Adopting the above interpretation of the various terms in the expression for a quaternion, the question next arises, in what sense are the lines represented by these terms, and by quaternions generally, said to be added? Now the fundamental formula for the addition of quaternions shows that in whatever way the line Q is formed from the quantities w, x, y, z (with similar expressions for any other quaternions $Q_1, Q_2 \dots$), then the sum ΣQ_n is a new quaternion line formed in the same manner from the quantities $\Sigma r_n, \Sigma x_n, \Sigma y_n, \Sigma z_n$; and writing

$$\Sigma Q_n = Q = (W, X, Y, Z),$$

it appears that W will be the algebraical sum of the lines $w, w_1, \dots$, supposed, for convenience, to be similarly directed,

and X, Y, Z will be the coordinates of the extremity of the diagonal of the parallelopiped formed on the sums of the component coordinates as its edges. From these two facts it appears that straight lines lying in the same straight line are to be added as in ordinary algebraical geometry, while the sum of any other set of straight lines inclined to one another at any angles is the closing side of the polygon formed by placing the beginning of each line at the termination of its predecessor. In fact, lines are to be added as forces are equilibrated in statics. In accordance with this principle, the sum

$$ix + jy + kz$$

will represent the diagonal of the parallelopiped described on the line ix, jy, kz as its edges; and since moreover

$$(ix + jy + kz)^2 = -(x^2 + y^2 + z^2) = -r^2,$$

therefore also

$$ix + jy + kz = (-)^{\frac{1}{2}}r, \quad . \quad . \quad . \quad . \quad (5.)$$

which, according to the principles of the present calculus, represents not merely a line in a plane perpendicular to r , but a line which has been brought into its position by means of a rotation through an angle $= \frac{\pi}{2}$ in that plane; or, in other words, about an axis whose direction-cosines are $x:r, y:r, z:r$; and finally, the sum

$$w + ix + jy + kz$$

will represent the diagonal of the square whose sides are

$$w + ix + jy + kz$$

(these two lines being obviously perpendicular). The length of the whole line is consequently

$$(w^2 + x^2 + y^2 + z^2)^{\frac{1}{2}} = \rho, \quad . \quad . \quad . \quad . \quad (6.)$$

and its direction makes an angle, whose tangent is $= r:w$, with the direction of w ; the whole quaternion will therefore represent a line whose length is ρ , which has been turned through an angle $= \tan^{-1}(r:w)$ in the plane, the direction-cosines of whose normal are $x:r, y:r, z:r$. The expression (1.) may also be written as follows:

$$\{\cos \theta + \sin \theta(il + jm + kn)\} \rho, \quad . \quad . \quad . \quad . \quad (7.)$$

where

$$\left. \begin{aligned} \theta &= \tan^{-1}(r:w) \\ x:l=y:m=z:n=r \end{aligned} \right\} . \quad . \quad . \quad . \quad (8.)$$

The following cases will exemplify the above interpretation of quaternions.

If ABC be a spherical triangle, the radius being equal to unity, and if Q, Q', Q'' indicate the rotations of the radius vector from B to C, from C to A, from A to B respectively, it is clear that we must always have

$$Q''Q'Q = QQ''Q' = Q'QQ'' = 1.$$

In order to find the quaternion which will represent the rotation from the line (l, m, n) to the line (l', m', n') , we may construct a quadrantal triangle such that (l, m, n) , (l', m', n') pass through the angles opposite to the quadrantal sides; and if Q be the required quaternion,

$$(li + mj + nk)(l'i + m'j + n'k)Q = -1;$$

but since

$$(li + mj + nk)^2 = (l'i + m'j + n'k)^2 = -1,$$

∴

$$(l'i + m'j + n'k)Q = (li + mj + nk)$$

$$-Q = (l'i + m'j + n'k)(li + mj + nk)$$

$$Q = -(l'l' + mm' + nn') + i(mn' - m'n) + j(nl' - n'l) + k(lm' - l'm).$$

To find the quaternions which will represent the rotations from the three coordinate axes to the line (l, m, n) , we need only put in the above equation,

$$m=0, n=0; n=0, l=0; l=0, m=0$$

in succession; hence, dropping the accents,

$$Q_x = -l - jn + km$$

$$Q_y = -m + in - kl$$

$$Q_z = -n - im + jl;$$

to which may be added the following relations:

$$Q_x^2 + Q_y^2 + Q_z^2 = -1$$

$$lQ_x + mQ_y + nQ_z = -1$$

$$iQ_x + jQ_y + kQ_z = -il - jm - kn$$

$$nQ_y - mQ_z = i - l(il + jm + kn)$$

$$lQ_z - nQ_x = j - m(il + jm + kn)$$

$$mQ_x - lQ_y = k - n(il + jm + kn)$$

$$(jm - kn)Q_x + (in - kl)Q_y + (jl - im)Q_z = -2$$

$$= (nQ_y - mQ_z)^2 + (lQ_z - nQ_x)^2 + (mQ_x - lQ_y)^2.$$

If Q, Q', Q'' be any quaternions, the condition

$$\alpha Q + \beta Q' + \gamma Q'' = 0$$

is equivalent to the system

$$\alpha w + \beta w' + \gamma w'' = 0$$

$$\alpha x + \beta x' + \gamma x'' = 0$$

$$\alpha y + \beta y' + \gamma y'' = 0$$

$$\alpha z + \beta z' + \gamma z'' = 0,$$

from the last three of which may be deduced

$$\begin{vmatrix} x, x', x'' \\ y, y', y'' \\ z, z', z'' \end{vmatrix} = 0$$

which is the condition that the three lines, whose direction-cosines are proportional to $x, y, z, \dots$, lie in the same plane; in other words, the planes of rotation of the three quaternions are all parallel to one straight line.

If Q, Q', Q'' represent the rotations BC, CA, AB of the spherical triangle ABC , the quaternions

$$\gamma Q' - \beta Q'' = Q_1$$

$$\alpha Q'' - \gamma Q = Q_2$$

$$\beta Q - \alpha Q' = Q_3$$

will represent arcs drawn from the angular points A, B, C , and cutting the opposite sides in points whose segments are in the ratios $\beta : \gamma, \gamma : \alpha, \alpha : \beta$ respectively, and the resulting condition

$$\alpha Q_1 + \beta Q_2 + \gamma Q_3 = 0$$

shows that the three planes of rotation intersect in a common line, for they all pass through the same point, viz. the centre of the sphere; consequently the three arcs all meet in a point. If

$$\alpha = \beta = \gamma,$$

the points where the arcs Q_1, Q_2, Q_3 meet the sides of the triangle will be the middle points of those sides, and the condition

$$Q_1 + Q_2 + Q_3 = 0$$

will express that the three arcs meet in a point. This theorem includes all the corresponding theorems with respect to plane triangles.

XI. *On the Structure and Arrangement of the Tesserae in a Roman pavement discovered at Cirencester in August 1849. By JAMES BUCKMAN, F.L.S., F.G.S.**

THE object of this paper is to point out the nature of the materials of which the party-coloured floors so beautifully wrought in ancient Roman dwellings are composed, as also to offer some remarks upon their principles of arrangement.

The tesserae of Roman pavements may be said to be formed out of two classes of materials, the first of which, consisting of portions of various coloured rocks, may be termed *natural*; the second, of stained or coloured terra cottas and glass, being *artificial*.

The natural tesserae furnish but few colours, and those of a sober cast, hence these will be found forming shadings to figures entering largely into the composition of borders, or filling up the groundworks of the designs. They consist of portions of natural rocks from various localities, those belonging to the district where the pavement is found, as far as I have observed, always contributing their share.

The Cirencester pavement presented the following :—

<i>Colours.</i>	<i>Rocks.</i>
1. White, composed of	Hard fine-grained Oolite.
2. Light yellow	Pebbles of the Wiltshire Drift, and Oolite.
3. Gray	The same as No. 1, altered by heat.
4. Slate colour or black ...	Limestone bands of the Lower Lias.

No. 1 occurs as a bed of compact fine-grained stone of about 2 feet thick in nearly all the freestone quarries of this district, where it is distinguished under the name of the Limestone bed; its geological position is about the middle of the freestone rocks of the Great Oolite; it is well exposed at Trewsbury quarry, at the Acman Street Station, and at the smaller Sapperton tunnel, and was no doubt obtained by the Romans from the quarries once worked by them in the vicinity of the Querns.

2. The tesserae, of a yellowish or nankeen hue, appear to have been made of portions of the pebble-drift with which parts of the neighbourhood of Cirencester is so thickly strewn. Stray pebbles of this may be found in almost every field to the south of the town, whilst at Somerford Kaynes, and other places, it enters largely into the composition of the gravel beds which are there worked. It is probable that this drift is the debris of that tertiary rock known in Wiltshire as Sarsen stone, of which the huge stones of Abury Camp constitute the more enduring monument.

3. This, though differing so much in colour from No. 1, yet

* Read to the Cotswold Club, Jan. 22, 1850.

seemed so identical in lithological structure as to induce me to try to ascertain from experiment whether or not they were the same, when, on roasting a portion of the rock No. 1 in the fire for a few minutes, it gradually assumed the colour of the gray tesserae, the change no doubt being due to some alteration in the chemical conditions of the iron with which the stone is slightly charged.

4. The dark colour of the lias entered largely into the composition of these pavements, as much of the outline of the design and the darker bands of the border ornaments are composed of this stone, which, judging from an Ammonite found in one of the tesserae, was obtained from some one of the thin layers of argillaceous limestone with which the clay-beds of the third division of the lower lias in the vale of Gloucester are separated, and no doubt the stone in question was brought from that locality.

The *artificial tesserae* found at Cirencester entered for the most part into the construction of the finer and more important parts of the details of the figures and designs; they consist of—

Colour.	Substance.
1. Black	} Terra cotta or baked clay.
2. Light red	
3. Dark red	
4. Brilliant ruby-red ...	Glass.

1. This is a much darker shade than that of the lias, and was consequently used in those portions of figures where bold relief was required; it seems to be composed of a dark-coloured clay, only slightly, if at all burnt; as the tesserae are very fragile, this would almost lead to the conclusion that these were not artificially coloured but made of a clay containing a large quantity of protoxide of iron, which is black, and they were consequently burnt in smother kilns, or the black would change to red by the protoxide becoming peroxidized. The identity of constitution of these tesserae with black pottery is very apparent.

2 and 3. The two reds are made from clays containing more or less of iron, and perhaps this substance may have been added in these and in the instance above noticed, where it was desirable to deepen the tint; of course the red is due to the peroxidation of the iron salts.

4. In only one medallion of the Cirencester pavements has glass been made to play a part, and that is just when the transparency and brilliancy of colour of this substance were of the utmost importance to the composition.

An examination of the pavement itself will show that the medallion which symbolized Spring, represents a fine female head crowned by what appears a chaplet of olive-green and verdigris-coloured leaves. Now on studying this head attentively, I was

surprised at seeing these two colours intermixed apparently in a most inharmonious manner; and as the verdigris-green was so different from any other colour I had met with, it suddenly struck me that it was a mere coating to the tesserae, resulting perhaps from chemical decomposition, and on scraping the surface with a knife I was gratified to find that the verdigris only covered up a glass of an exceedingly rich ruby tint. I then obtained a small portion for a chemical analysis, which was kindly undertaken for me by Dr. Voelcker, the College Professor of Chemistry, the results of which are so interesting that I must beg to lay it before the Club in his own words.

Examination of red-coloured Roman glass (Cirencester).

“The red glass which had undergone a partial decomposition was coated with a white crust, which itself was covered with a green substance. The latter on examination proved to be carbonate of copper; the white crust dissolved with effervescence in nitric acid, leaving gelatinous silicic acid behind, and was found to consist principally of carbonate of lead and silica. The glass, after having been treated with nitric acid and thus been deprived of the white and green coatings, exhibited a bright red colour; it was now transparent, not very hard, and easily flexible when exposed to a moderate heat. On analysis the following substances were detected, as—

Oxide of lead.	Oxide of iron.
Protoxide of copper.	Lime.
Alumina.	Silica.

“The red colour of the glass undoubtedly is due to protoxide of copper, which was present probably in combination with alumina in considerable quantities. It is well known that the ancients were acquainted with the art of colouring glass red by means of copper, for Cooper informs us (*Annales de Chimie*, série 1. tom. lxxiii. p. 20) that he detected in an antique red glass protoxide of copper, and Klaproth likewise ascribes the red colour of an antique glass to the presence of copper, which he considers to be contained in the glass in the state of protoxide. This gentleman found exactly the same constituent parts as those found by me in the Cirencester glass; it has further been ascertained that all the red glass in antique mosaic church windows is coloured red by copper. Gold, which likewise imparts a beautiful red colour to glass, is never met with in Roman glass, and it appears that the property of gold and its combinations was unknown to the Romans, for we do not find any traces with the ancients which could justify the supposition of their being acquainted with the art of making the purple and rose-coloured or

ruby-glass which at present is manufactured in great perfection in Bohemia, where a preparation of gold, generally chloride of gold, is used for that purpose by the glass manufacturer. The application of gold preparations in the preparation of red glass, comparatively speaking, is of recent origin, for it appears that before the 17th century the use of gold preparation for this particular purpose was unknown. In the 17th century we find the first reference made to the use of gold for colouring glass red by Cassius, who discovered and recommended a new combination of gold, which, to the present day, is known under the name of Cassius gold purple.

"Copper thus appears to have been the material with which the ancients were in the habit of colouring glass red. Various methods of applying copper were in use, and though metallic copper is capable of imbuing glass with a red colour, no doubt on account of the protoxide of copper which is found in almost every sample of copper, in most cases it was first subjected to operations which tended to generate protoxide of copper. Frequently also peroxide of copper (black oxide) was used for the same purpose, but in this case the glass mass received an addition of substances, as tartar, charcoal, soot, iron, protoxide of iron, which substances at a red heat combine with part of the oxygen of the black oxide, and thus become the means of reducing the latter to red oxide of copper.

"This important action of iron seems to have been known to the ancients, for both Cooper's and Klaproth's analyses of antique red glass referred to above, as well as my own of the Roman glass found in Cirencester, exhibits, besides oxide of copper, oxide of iron. Later the art of colouring glass red by means of copper was lost entirely, and many persons of our days even denied altogether the possibility of producing a red glass with copper. Very generally all red glass was supposed to contain gold.

"The importance of the subject induced the Society of Arts of Berlin to offer a prize for a method of manufacturing red glass by means of copper. The prize was gained by D. Engelhardt of Zinswider, who gave several directions of manufacturing red glass, and who succeeded in making a beautiful red glass with protoxide of copper and without using gold at all." (*Vide Verhandl. des Gewerbevereins, Berlin, 1828, S. 15.*)

From this analysis it will be seen that the Romans imparted this red tint to glass by a very ingenious method, and it was the substance used for this purpose, namely copper, which covered over the tesserae as the surface of the glass had become decomposed in the form of a carbonate of that metal.

This fact is curious in its bearing upon the pavement as a work of art; as so harmoniously are the colours arranged in all the

figures that it may almost be taken for granted that, as in this instance, when there is an exception in this particular, it is due to some subsequent change having taken place in one or other of the colours. In the case before us our first tracing was coloured with the verdigris-green: it was unsatisfactory; but on making a new tracing and colouring it according to our amended observations, it at once became harmonious in colour, and assumed an intelligible form, though all our colouring will not enable us to convey the idea of ruby-gemmed flowers like the substance used, the transparency of glass contributing much to the general effect.

XII. *The Effect of Pressure in Lowering the Freezing-Point of Water experimentally demonstrated.* By Professor W. THOMSON, Glasgow*.

ON the 2nd of January 1849, a communication entitled "Theoretical Considerations on the Effect of Pressure in Lowering the Freezing-Point of Water, by James Thomson, Esq., of Glasgow," was laid before the Royal Society, and it has since been published in the Transactions, vol. xvi. part 5†. In that paper it was demonstrated that, if the fundamental axiom of Carnot's Theory of the Motive Power of Heat be admitted, it follows, as a rigorous consequence, that the temperature at which ice melts will be lowered by the application of pressure; and the extent of this effect due to a given amount of pressure was deduced by a reasoning analogous to that of Carnot from Regnault's experimental determination of the latent heat, and the pressure of saturated aqueous vapour at various temperatures differing very little from the ordinary freezing-point of water. Reducing to Fahrenheit's scale the final result of the paper, we find

$$t = n \times 0.0135;$$

where t denotes the depression in the temperature of melting ice produced by the addition of n "atmospheres" (or n times the pressure due to 29.922 inches of mercury), to the ordinary pressure experienced from the atmosphere.

In this very remarkable speculation, an entirely novel physical phenomenon was *predicted* in anticipation of any direct experiments on the subject; and the actual observation of the phenomenon was pointed out as a highly interesting object for experimental research.

* From the Proceedings of the Royal Society of Edinburgh, February 1850.

† It will appear also, with some slight alterations made by the author, in the Cambridge and Dublin Mathematical Journal, Nov. 1850.—W. T.

To test the phænomenon by experiment without applying excessively great pressure, a very sensitive thermometer would be required, since for ten atmospheres the effect expected is little more than the tenth part of a Fahrenheit degree; and the thermometer employed, if founded on the expansion of a liquid in a glass bulb and tube, must be protected from the pressure of the liquid, which, if acting on it, would produce a deformation, or at least a compression of the glass that would materially affect the indications. For a thermometer of extreme sensibility, mercury does not appear to be a convenient liquid; since, if a very fine tube be employed, there is some uncertainty in the indications on account of the irregularity of capillary action, due probably to superficial impurities, and observable even when the best mercury that can be prepared is made use of; and again, if a very large bulb be employed, the weight of the mercury causes a deformation which will produce a very marked difference in the position of the head of the column in the tube according to the manner in which the glass is supported, and may therefore affect with uncertainty the indications of the instrument. The former objection does not apply to the use of any fluid which perfectly wets the glass; and the last-mentioned source of uncertainty will be much less for any lighter liquid than mercury, of equal or greater expansibility by heat. Now the coefficient of expansion of sulphuric æther at 0° C. being, according to M. I. Pierre*, $\cdot 00151$, is eight or nine times that of mercury (which is $\cdot 000179$, according to Regnault), and its density is about the twentieth part of the density of mercury. Hence a thermometer of much higher sensibility may be constructed with æther than with mercury, without experiencing inconvenience from the circumstances which have been alluded to. An æther thermometer was accordingly constructed by Mr. Robert Mansell of Glasgow, for the experiment which I proposed to make. The bulb of this instrument is nearly cylindrical, and is about $3\frac{1}{2}$ inches long and $\frac{3}{8}$ ths of an inch in diameter. The tube has a cylindrical bore about $6\frac{1}{2}$ inches long: about $5\frac{1}{2}$ inches of the tube are divided into 220 equal parts. The thermometer is entirely inclosed, and hermetically sealed in a glass tube, which is just large enough to admit it freely†. On comparing the indications of this instru-

* See Dixon on Heat, p. 72.

† Following a suggestion made to me by Professor Forbes of Edinburgh, I have in subsequent experiments with this thermometer, used it with enough of mercury introduced into the tube in which it is hermetically sealed to entirely cover its bulb; as I found that, without this, if the experiment was conducted in a warm room, the indications of the thermometer were frequently deranged by the portion of the water which was left free from ice becoming slightly elevated in temperature.

ment with those of a thermometer of Crichton's with an ivory scale, which has divisions corresponding to degrees Fahrenheit of about $\frac{1}{25}$ th of an inch each, I found that the range of the æther thermometer is about 3° Fahrenheit; and that there are about 212 divisions on the tube corresponding to the interval of pressure from 31° to 34° , as nearly as I could discover from such an unsatisfactory standard of reference. This gives $\frac{1}{71}$ of a degree for the mean value of a division. From a rough calibration of the tube which was made, I am convinced that the values of the divisions at no part of the tube differ by more than $\frac{1}{30}$ th of this amount from the true mean value; and, taking into account all the sources of uncertainty, I think it probable that each of the divisions on the tube of the æther thermometer corresponds to something between $\frac{1}{68}$ and $\frac{1}{75}$ of a degree Fahrenheit.

With this thermometer in its glass envelope, and with a strong glass cylinder (Cæsted's apparatus for the compression of water), an experiment was made in the following manner:—

The compression vessel was partly filled with pieces of clean ice and water: a glass tube about a foot long and $\frac{1}{10}$ th of an inch internal diameter, closed at one end, was inserted with its open end downwards, to indicate the fluid pressure by the compression of the air which it contained; and the æther thermometer was let down and allowed to rest with the lower end of its glass envelope pressing on the bottom of the vessel. A lead ring was let down so as to keep free from ice the water in the compression cylinder round that part of the thermometer tube where readings were expected. More ice was added above; so that both above and below the clear space, which was only about two inches deep, the compression cylinder was full of pieces of ice. Water was then poured in by a tube with a stopcock fitted in the neck of the vessel, till the vessel was full up to the piston, after which the stopcock was shut.

After it was observed that the column of æther in the thermometer stood at about 67° , with reference to the divisions on the tube, a pressure of from 12 to 15 atmospheres was applied, by forcing the piston down with the screw. Immediately the column of æther descended very rapidly, and in a very few minutes it was below 61° . The pressure was then suddenly removed, and immediately the column in the thermometer began to rise rapidly. Several times pressure was again suddenly applied, and again suddenly removed, and the effects upon the thermometer were most marked.

The fact that the freezing-point of water is sensibly lowered by a few atmospheres of pressure was thus established beyond

all doubt. After that I attempted, in a more deliberate experiment, to determine as accurately as my means of observation allowed me to do, the actual extent to which the temperature of freezing is affected by determinate applications of pressure.

In the present communication I shall merely mention the results obtained, without entering at all upon the details of the experiment.

I found that a pressure of, as nearly as I have been able to estimate it, 8·1 atmospheres produced a depression measured by $7\frac{1}{2}$ divisions of the tube on the column of æther in the thermometer; and again, a pressure of 16·8 atmospheres produced a thermometric depression of $16\frac{1}{2}$ divisions. Hence the observed lowering of temperature was $\frac{7\frac{1}{2}}{71}$, or $\cdot 106^{\circ}$ F. in the former case, and $\frac{16\frac{1}{2}}{71}$, or $\cdot 232^{\circ}$ F. in the latter.

Let us compare these results with theory. According to the conclusions arrived at by my brother in the paper referred to above, the lowering of the freezing-point of water by 8·1 atmospheres of pressure would be $8\cdot 1 \times \cdot 0135$, or $\cdot 109^{\circ}$ F.; and the lowering of the freezing-point by 16·8 atmospheres would be $16\cdot 8 \times \cdot 0135$, or $\cdot 227^{\circ}$ F. Hence we have the following highly satisfactory comparison, for the two cases, between the experiment and theory:—

Observed pressures.	Observed depressions of temperatures.	Depressions according to theory, on the hypothesis that the pressures were truly observed.	Differences.
8·1 atmospheres ...	$\cdot 106^{\circ}$ F.	$\cdot 109^{\circ}$ F.	$-\cdot 003^{\circ}$ F.
16·8 atmospheres ...	$\cdot 232^{\circ}$ F.	$\cdot 227^{\circ}$ F.	$+\cdot 005^{\circ}$ F.

It was, I confess, with some surprise, that, after having completed the observations under an impression that they presented great discrepancies from the theoretical expectations, I found the numbers I had noted down indicated in reality an agreement so remarkably close, that I could not but attribute it in some degree to chance, when I reflected on the very rude manner in which the quantitative parts of the experiment (especially the measurement of the pressure, and the evaluation of the division of the æther thermometer) had been conducted.

I hope before long to have a thermometer constructed, which shall be at least three times as sensitive as the æther thermometer I have used hitherto; and I expect with it to be

able to perceive the effect of increasing or diminishing the pressure by less than an atmosphere, in lowering or elevating the freezing-point of water.

If a convenient *minimum* thermometer could be constructed, the effects of very great pressures might easily be tested by hermetically sealing the thermometer in a strong glass, or in a metal tube, and putting it into a mixture of ice and water, in a strong metal vessel, in which an enormous pressure might be produced by the forcing-pump of a Bramah's press.

In conclusion, it may be remarked, that the same theory which pointed out the remarkable effect of pressure on the freezing-point of water, now established by experiment, indicates that a corresponding effect may be expected for all liquids which expand in freezing; that a reverse effect, or an elevation of the freezing-point by an increase of pressure, may be expected for all liquids which contract in freezing; and that the extent of the effect to be expected may in every case be deduced from Regnault's observations on vapour (provided that the freezing-point is within the temperature-limits of his observations), if the latent heat of a cubic foot of the liquid, and the alteration of its volume in freezing be known.

XIII. On a remarkable appearance of Lightning.

By J. P. JOULE, F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ON the 16th inst., after a very sultry morning, this town was, in common with a large tract of country, visited at 4 o'clock by a thunder-storm accompanied with heavy rain. In the evening of the same day, about 9 o'clock, we had an opportunity of witnessing a most magnificent display of electrical discharges, which continued almost uninterruptedly for the space of one hour, accompanied, however, by only a few drops of rain. I had never before seen lightning of such an extraordinary character. Each discharge appeared to emanate from a mass of clouds in the south-west, and travelled six or ten miles in the direction of the spectator, dividing into half a dozen or more sparks, or zigzag streams of light, in some instances the termination of each of these sparks being, as represented in the adjoining sketch, again subdivided into a number of smaller sparks. I did not observe any of the discharges to strike the ground; and from the interval of time between the appearance of those which crossed the zenith and

the thunder, I consider that their general elevation from the surface of the earth must have been at least $3\frac{1}{2}$ miles.

The diverging form arose no doubt from the extensive negative surface presented by the clouds, and may be imitated on a small scale by filling a glass jar with water and using it as a Leyden phial. If such a jar be discharged by bringing one ball of the discharging rod towards the exterior glass surface, the other ball being in connexion with the water, the spark will, in restoring the electrical equilibrium, diverge over the whole glass surface.



Another remarkable feature in the lightning was the *sensible time* of its travelling towards the spectator. The main streams of light were always formed before the diverging sparks; and when formed, remained steady for an appreciable time, until the whole disappeared together. My brothers, Messrs. Benjamin and John Joule, who observed the lightning two miles westward of my station, formed exactly the same impression of its character; and in addition, the latter and several other parties were witnesses of a phænomenon, which, if owing to the electrical state of the atmosphere, was, I believe, without a recorded precedent.

At half-past 8 o'clock a bright red light appeared among the clouds, bearing nearly due south, and having an elevation of about 30° above the horizon. It appeared as if the sun were behind a cloud illuminating its edges strongly, and throwing a brilliant light upon the neighbouring clouds. It lasted about five minutes with perfect steadiness, and then gradually disappeared.

I ought to mention, that, during the above-described phænomena, violent thunder-storms were taking place in different parts of this county and in Cheshire, but without any apparent connexion with them.

I have the honour to remain, Gentlemen,

Yours very respectfully,

Acton Square, Salford, Manchester,
July 19, 1850.

JAMES P. JOULE.

XIV. *Remarks on the Weather during the Quarter ending June 30, 1850.* By JAMES GLAISHER, Esq., F.R.S., Hon. Sec. of the British Meteorological Society, &c.*

THE weather during the past quarter has been variable, and at times very unusual. The temperature of the air till April 21 was $4^{\circ}3$ above the average, and this period was free from frosts. From April 22 to May 16, there was an average deficiency of 5° of daily temperature; from May 17 to June 9, the temperature was about its average value; it was 8° in excess on June 11, and 13° in defect on the 15th; and during the following night the temperature of the air in many places was below 32° , a very unusual circumstance for the season. From June 18 to the 26th, the period was warm; the mean excess of temperature was 6° . Snow has fallen on several days during the past quarter.

The mean temperature of the air at Greenwich for the three months ending May, constituting the three spring months, was $46^{\circ}6$, being of almost the same value as that of the average from the seventy-nine preceding springs.

For the month of April was $48^{\circ}5$, exceeding that of the average of the preceding seventy-nine years by $2^{\circ}8$, and exceeding that of the preceding nine years by $1^{\circ}0$.

For the month of May was $51^{\circ}3$, being $1^{\circ}3$ less than the average of the preceding seventy-nine years, and $3^{\circ}1$ less than that of the preceding nine years.

For the month of June was $60^{\circ}8$, exceeding that of the average of the preceding seventy-nine years by $2^{\circ}8$, and exceeding that of the preceding nine years by $1^{\circ}2$.

The mean for the quarter was $53^{\circ}4$, exceeding that of the average of seventy-nine years by $1^{\circ}4$, and being less than that of the preceding nine years by $0^{\circ}3$.

The mean temperature of evaporation at Greenwich—

For the month of April was $45^{\circ}4$; for May was $47^{\circ}5$; and for June was $54^{\circ}8$. These values are $1^{\circ}7$ greater, $3^{\circ}0$ less, and $0^{\circ}1$ greater than those of the averages of the same months in the preceding nine years.

The mean temperature of the dew-point at Greenwich—

For the months of April, May and June, were $41^{\circ}7$, $43^{\circ}4$, and $50^{\circ}1$ respectively. These values are $1^{\circ}0$ greater, $4^{\circ}0$ less, and $1^{\circ}8$ less respectively than the averages of the same months in the preceding nine years.

The mean elastic force of vapour at Greenwich for the quarter was 0.318 inch, being less than the average from the preceding nine years by 0.031 inch.

* Communicated by the Author.

The mean weight of water in a cubic foot of air for the quarter was 3·6 grains. The average from the preceding nine years was 3·8 grains.

The mean degree of humidity in April was 0·795, in May was 0·765, and in June was 0·702. The averages from the nine preceding years were 0·808, 0·788 and 0·702 respectively.

The mean reading of the barometer at Greenwich in April was 29·594 inches, in May was 29·714, and in June was 29·886. These readings are 0·114 less, 0·071 less, and 0·089 greater respectively than the averages of the same months in the preceding nine years.

The average weight of a cubic foot of air for the quarter, under the average temperature, humidity and pressure, was 532 grains; being of the same value as that of the average of the preceding nine years.

The rain fallen at Greenwich in April was 2·4 inches, in May was 2·3, and in June was 1·0. The falls for these three months on an average of thirty-four years, are 1·7, 2·0 and 1·7 inches respectively.

The average daily ranges of the readings of the thermometer in air at the height of four feet above the soil, in April was 16°·0, in May was 18°·9, and in June was 26°·0. The averages for these three months from the preceding nine years were 17°·4, 18°·9 and 19°·4 respectively.

The minimum readings of the thermometer on grass in April was at or below 32° on twelve nights; the lowest was 23°; was between 32° and 40° on fourteen nights, and exceeded 40° on four nights; the highest reading was 44°. In May the readings were at and below 32° on thirteen nights; the lowest was 15°; they were between 32° and 40° on eleven nights; and on seven nights the readings exceeded 40°. In June the readings were at or below 32° on two nights; the lowest was 29°; they were between 32° and 40° on six nights, and they exceeded 40° on twenty-two nights. At Cardington, as observed by S. C. Whitbread, Esq., the reading of the thermometer on grass in April was twelve nights, in May was twelve nights, and in June was three nights below 32°.

The temperature of the water of the Thames, from the observations of Lieut. Sanders, R.N., Superintendent of the Dreadnought Hospital Ship, was 48°·4 in April, 54°·3 in May, and 63°·7 in June.*

Fog was prevalent on April 6 at Dundee and Whitby; on the 12th at Rugby and Whitby; on the 13th at Folkestone and Whitby; on the 16th at Whitby and Dundee; and on the 20th at Edinburgh, Berwick and Whitby. On May 8 at Stone and Whitby; on the 11th at Greenwich; on the 15th,

16th and 17th, at Durham; on the 18th at Hastings; on the 20th at Hartwell House, Hartwell Rectory, Durham and Whitby; on the 21st at Hartwell House, Hartwell Rectory, Berwick, Whitby and Durham; on the 22nd at Brighton, Darlington, Durham, Dundee, Berwick and Whitby; on the 23rd at Darlington, Durham, Dundee and Edinburgh; on the 24th at Hartwell House, Darlington, Yarmouth and Whitby; on the 25th at Whitby; and on the 31st at Berwick and Hartlepool. On June 1 at Berwick, Sunderland and Dundee; on the 2nd at Berwick; on the 3rd and 4th at Edinburgh; on the 5th at Folkestone; on the 17th at Sunderland; on the 18th at Glasgow; and on the 20th at Whitehaven.

Meteors.—At Stone, on April 10, at 10^h P.M., a meteor shot from Jupiter to γ Leonis. On May 2, at 10^h P.M., a meteor shot from Virgo about 4° from Jupiter, and went as far as Jupiter; on May 29, at 10^h 5^m P.M., a meteor shot from α Cygni southwards. On June 4, at 11^h 28^m P.M., a meteor shot from α Urs. Min. (Polaris) to δ Urs. Maj.; on the 16th, at 0^h 25^m A.M., a meteor shot from the west of β Cassiopeæ and went 4° north; on the same night, at 0^h 40^m A.M., a splendid meteor, larger than a star of the first magnitude, shot from the west of Capella 10° east of due north, and about 15° above the horizon, and went in a westward direction near to the star 31 Lyncis, leaving a train of blue light of about 20°; a few seconds after a small meteor shot from above Polaris to Cassiopea; on the same night, at 0^h 45^m A.M., a meteor shot from β Serpentis, and went about 5° south; at 1^h 3^m A.M., a meteor shot from ϵ Bootis to Arcturus; at 1^h 20^m A.M., a meteor as large as a star of the first magnitude, and of a beautiful red colour, shot from ϵ Urs. Maj. and passed by α Urs. Maj. On the 20th, at 11^h 42^m P.M., a meteor shot from α Lyræ to α Cygni; on the 24th, at 11^h 30^m P.M., a meteor as large as a star of the first magnitude shot from Arcturus and went 20° magnetic west, leaving a train of blue light.

On June 4, at Hartwell Rectory, a small meteor was seen from Polaris to the Pointers, at 11^h 30^m; on the 21st a meteor shot from α Lyræ to α Cygni at 11^h 42^m P.M.* On June 24 a meteor was seen from Arcturus to within 10° of the horizon at 11^h 30^m.

At Nottingham, on May 1, at 10^h 33^m, a meteor of the size of a star of the second mag. fell slowly from 30° above S. horizon, at an angle of 40° to west; and at 11^h 10^m another fell downwards 5° south of Jupiter. May 30, at 10^h 38^m, a

* This is evidently the same meteor as that observed at Stone, but which is referred to June 20: which day is right?

meteor, size second mag., colour yellow, passed nearly horizontally $1\frac{1}{2}^{\circ}$ under Vega, moving to south. June 1, globe meteor, size of Jupiter but less bright, of a red colour, having a well-defined disc, moved from γ through ϕ Cassiopeæ, ended 3° east of α Persei, duration $1\frac{1}{2}$ minute. On the 3rd, another size, third mag., blue colour, ill-defined, passed from α Cygni through Lacerta at $10^{\text{h}} 30^{\text{m}}$; and at $10^{\text{h}} 45^{\text{m}}$ a nearly similar one from λ Draconis through η Draconis.

Solar Halos were seen on April 1 at Greenwich; on the 2nd at Stone and Hartwell Rectory; on the 7th at Greenwich and Stone; on the 14th at Stone; on the 17th at Nottingham; on the 18th at Guernsey, Greenwich and Nottingham; on the 19th at Stone and Nottingham; on the 21st at Hartwell Rectory; and on the 25th at Greenwich and Nottingham. On May 4 at Durham; on May 5 at Uckfield; on May 7 at Durham; on May 13 at Uckfield; on May 14 at Hartwell Rectory; on May 19 at Durham; on May 23 and 26 at Hartwell Rectory; and on May 28 at Nottingham. On June 2 at Nottingham and Whitehaven; on the 3rd at Nottingham; on the 4th at Greenwich, Stone, Hartwell Rectory, Nottingham, Stonyhurst and Durham; on the 5th at Stone; on the 8th at Hartwell House; on the 9th at Stone, Rose Hill, Oxford, and Nottingham; on the 10th at Southampton, Stone, Hartwell House, Cardington, Rose Hill, Oxford, Norwich and Nottingham; on the 11th at Stone, Rose Hill, Oxford and Nottingham; on the 12th at Nottingham; on the 14th at Cardington; on the 16th at Stone, Rose Hill, Oxford and Nottingham; on the 17th at Stone and Aylesbury; on the 18th at Aylesbury; on the 20th at Stone and Nottingham; on the 21st at Stone and Nottingham; and on the 29th at Uckfield and Nottingham.

Lunar Halos were seen on April 16 and 17 at Hartwell Rectory; on the 19th at Wakefield; on the 20th at Liverpool; on the 21st, 22nd, 23rd, at Hartwell Rectory; on the 24th at Stonyhurst. On May 20 and 22 at Uckfield; and on the 26th at Stone. On June 16 at Stone and Hartwell Rectory; on the 18th at Stone; on the 20th at Guernsey, Stone, Hartwell Rectory, and Radcliffe Observatory, Oxford; on the 21st at Jersey, Stone and Hartwell Rectory; and on the 25th at Uckfield.

Paraselenæ were seen on May 28 at Durham; and on June 20 and 21 at Stone and Hartwell Rectory.

Perihelion was seen on May 21 at Nottingham.

Auroræ boreales were seen on April 5 at Whitehaven; on April 6 at Durham; on May 12 at Aylesbury, Oxford, Stonyhurst and Durham; on June 5 at Highfield House, Notting-

ham ; on June 13 at Hartwell House, Radcliffe Observatory, Oxford, and at Rose Hill near Oxford ; on June 26 near Manchester ; and on June 27 at Nottingham and at Chesterfield.

Thunder-storms occurred on April 2 at Wakefield, Leeds, Liverpool, Stonyhurst and Whitehaven ; on the 8th at Uckfield ; on the 10th at Aylesbury ; on the 11th at Hartwell Rectory, Stone, Cardington and Saffron Walden ; on the 12th at Uckfield, Greenwich, London and Saffron Walden ; on the 13th at Greenwich ; on the 17th at Norwich ; on the 20th at Holkham, Nottingham and Exeter ; on the 23rd at Hawarden. On May 7 at Uckfield ; on the 13th at Leeds and Hawarden ; on the 17th at Uckfield ; on the 19th at Derby ; on the 22nd at Stonyhurst ; on the 23rd at Stone, Hartwell Rectory, Hartwell House, Leinslade, Bucks, Rose Hill, Oxford, Cardington, Saffron Walden, Derby, Nottingham, Liverpool, Leeds and Manchester ; on the 24th at Hartwell Rectory, Stone, Hartwell House, Rose Hill, Oxford, and Radcliffe Observatory, Oxford ; on the 26th at Norwich ; on the 27th at Leeds, Manchester, Durham and North Shields ; on the 30th at Hartwell House, Liverpool and Stonyhurst ; on the 31st at Stone and Rose Hill, Oxford. On June 5 at Wakefield, North Shields and Durham ; on the 6th at Hartwell House, Hartwell Rectory, Leeds, Stonyhurst, Durham and Whitehaven ; on the 7th at Leeds ; on the 12th at Helston ; on the 13th at Uckfield ; on the 16th at Durham ; on the 17th at North Shields ; on the 25th at Wakefield and Leeds ; on the 26th at Guernsey, Helston, Falmouth, Truro, Exeter, Uckfield, Southampton, St. John's Wood, Greenwich, Stone, Aylesbury, Hartwell House, Hartwell Rectory, Leinslade, Bucks, Saffron Walden, Radcliffe Observatory, Oxford, and Cardington ; on the 27th at Guernsey, Jersey, Exeter, Chichester, Uckfield, Helston, Southampton, St. John's Wood, and Hartwell Rectory ; on the 28th at Greenwich, Chichester, St. John's Wood, Uckfield and Hartwell House.

Of these storms that of the 26th of June was the worst. It was described by J. Johnson, Esq., of Oxford Observatory, as the most violent storm of thunder and lightning ever remembered there. It began about 2^h 30^m P.M., and lasted till about 4^h 30^m P.M. Two college towers were struck by lightning. No life was lost ; but he had heard of five persons (three children) who were thrown down by the violence of the lightning. There appears to have been two storms, one succeeding the other after an interval of about thirty minutes. I was not here myself, but the storm has been described to me by two trustworthy persons as terrific. As far as I can make out, the storm passed over the town in a N.N.E. direction.

At Hartwell Rectory, the Rev. C. Lowndes states, "that on

the 26th thunder was heard at 1^h 30^m P.M., and at 3 P.M. there was a heavy storm with thunder and lightning: it continued stormy during the evening and night."

At Hartwell House, Mr. Horton says, "that on June 26 a mansion near Thame, called Thame House, about ten miles from here, was set on fire by the lightning."

At Truro, Dr. C. Barham says, "the thunder-storm on June 26 was rather severe, but more so a few miles to the northward. Eleven sheep were killed by the lightning in one field, and four in a neighbouring one about ten miles to the north-east. The rain was not very heavy, and there was no hail: there was a fall of 16° of temperature between 1^h and 5^h P.M., and the weather has continued unsettled with showers and squally from that time to the present, July 3."

At Exeter, Dr. Shapter remarks, "that for three days previously to June 26 the atmosphere had gradually become hot and sultry, and at 4 P.M. on this day it became extremely oppressive. Distant thunder was then heard, and heavy rain-clouds came up with a light wind from the south. At 6 P.M. the storm reached Exeter; the lightning was constant and vivid, and heavy rain fell for two hours, when the storm moderated and passed on, and the wind shifted rather suddenly till 6 P.M. It reached Bridgewater at about 9 P.M. The electric telegraph on the South Devon Railway was rendered useless for several hours; the trains were consequently delayed, and considerable inconvenience was occasioned. The general character of the other parts of the month was fine and warm. Rain to the depth of 1·21 inch fell during the storm."

At Uckfield, C. L. Prince, Esq. says, "that on the 26th, at night, the electric fluid struck a house in this place and shattered a portion of the roof, burnt some clothes, &c.; and injured no one, although there were thirty persons under the roof at the time."

At Southampton rain to the depth of 1·96 inch fell during the passage of the storm.

Thunder was heard, but lightning was not seen, on April 11 at Rose Hill, Oxford, and Saffron Walden; on the 12th at Saffron Walden and Norwich; on the 17th at Hartwell House; on the 20th and 21st at Nottingham. On May 7 at Guernsey; on the 13th at Cardington, Stone and Aylesbury; on the 17th at Nottingham; on the 18th at Wakefield and Nottingham; on the 19th at Cardington and Nottingham; on the 21st at Exeter and Hawarden; on the 22nd at Aylesbury and Holkham; on the 23rd at Aylesbury, Norwich, Holkham, Oxford, Wakefield and Stonyhurst; on the 24th at Cardington and Hawarden; on the 25th and 26th at Hawarden; on the 27th at Guernsey, Wakefield and Stonyhurst; and on

the 31st at Hartwell Rectory, Leinslade, Bucks, Cardington, Oxford, Liverpool, Stonyhurst and Whitehaven. On June 5 at Nottingham and Dundee; on June 6 at Stone and Nottingham; on June 7 at Nottingham; on June 9 and 11 at Stone; on June 12 at Helston; on June 16 at Stonyhurst; on June 25 at Nottingham; on June 26 at Jersey, St. John's Wood, Wakefield and Nottingham; on the 27th at Stonyhurst; and on the 28th at Jersey.

Lightning was seen, but thunder was not heard, on April 2 at Stone and Stonyhurst; and on the 20th at Nottingham. On May 2 at Stone; on the 8th at Guernsey; and on the 30th at St. John's Wood. On June 5 at Nottingham; on the 24th at Nottingham; on the 25th at Cardington and Nottingham; on the 26th at St. John's Wood; on the 27th at St. John's Wood and Aylesbury; on the 28th at Aylesbury and Cardington; on the 29th at Aylesbury.

Hail fell on April 3 at Liverpool; on the 4th at Stone and Liverpool; on the 15th at Liverpool; on the 17th at Nottingham; on the 20th at Truro, Exeter, Saffron Walden, Cardington, Holkham, Norwich; on the 21st and 22nd at Nottingham; on the 25th at Rose Hill, Oxford; and on the 30th at Guernsey. On May 1 at Holkham and Saffron Walden; on the 4th at Leinslade, Bucks, Saffron Walden and Cardington; on the 5th at Oxford, Wakefield, Durham and North Shields; on the 6th at Guernsey, North Shields and Durham; on the 9th at Uckfield; on the 14th at Rose Hill, Oxford, and Durham; on the 15th at Greenwich, Aylesbury, Stone, Hartwell House, Leinslade, Bucks, Cardington, Radcliffe Observatory, Oxford, Saffron Walden, Stonyhurst, Whitehaven, Durham and North Shields; on the 17th at Uckfield; on the 23rd at Stone, Cardington and Nottingham; on the 31st at Hartwell Rectory. On June 6 at Nottingham and Stonyhurst; on the 10th at Uckfield, Hartwell House and Yarmouth; on the 16th at Durham; on the 17th at Uckfield; on the 19th at Aylesbury; on the 26th at Helston, Stone, and Rose Hill, Oxford; on June 27 at Uckfield.

Snow fell at Aylesbury on April 29; at North Shields, Whitehaven, Beattock and Edinburgh, on May 5; at Leeds on May 8; at Wakefield on May 9; at North Shields on May 15; at London on the 16th; and at Stone on the 23rd. These falls of snow in May are very unusual.

The horizontal movement of the air at Greenwich in April was 110 miles, in May 96 miles, and in June was 90 miles daily.

The series of observations of the *direction of the wind*, taken at the various railway stations, and published by the *Daily News*, has continued with great regularity; and the following Tables have principally been formed from them:—

April 1850.	Direction of the Wind.								General Remarks.
	On the south coast.	On the south-east coast.	On the east coast.	On the north-east coast.	On the north-west coast.	On the south-west coast.	In the southern counties.	In the midland counties.	
1	s.e.	s.	s.	s.w.	s.e.	s.	s.e.	s.e.	Gentle breeze. Rain at a few places.
2	s. & s.e.	s.	s.	s. & s.e.	s. & s.e.	s.	s.	s.e. & e.	Air in gentle motion. Rain to the North.
3	s. & s.w.		s.	s.w.	s.	s.w.	s. & s.w.	s. & w.	Wind variable in strength.
4	variable.	s.w.	s.	variable.	variable.	s.w.	s.w. & s.	variable.	Heavy storms of wind and rain South of Birmingham.
5	w. & n.w.	w.	s.w.	variable.	n.w. & w.	n.w. & w.	w.	w. & n.w.	Strong breeze and hard wind everywhere.
6	s.w.	s.w.	s.	w. & s.w.	s.	s. & s.w.	s. & s.e.	variable.	Strong breeze to the South. Rain in many places.
8	variable.	e.	s.	s. & e.	s. & s.e.	e.	s.e.	variable.	Air in gentle motion.
9	variable.	s.w.	s.	s.w.	s.w.	s.w.	s.	s.	A strong breeze at every place North of Whitby.
10	variable.	s.w.	s.w.	s.w.	s.e.	s.e.	s.	s.	A gentle breeze at most places.
11	s. & s.e.	s.e.	e.	variable.	variable.	s. & s.e.	s. & s.e.	e.	Gentle breeze and rain to the South. Calm and gentle breeze [to the North.
12	e.	s.	s.e.	n.e. & e.	e.	variable.	variable.	e.	Calm and gentle breeze.
13	variable.	s.e.	n.w.	s.e.	s.e.	variable.	e.	e.	Calm at most places.
15	s.w.	s.e.	s.e.	s.e.	e.	s.w.	s.e.	e.	Hard wind at most places. A gale and rain at Crewe.
16	variable.	s.	s.w.	s.	s.e.	variable.	s.e. & s.	e.	A hard wind and rain at most places North of Crewe.
17	w.	s.w.	s.w.	w.	variable.	s.w.	w.	variable.	A strong breeze at many places.
18	n.	n.w.	n.w.	w.	s.w.	variable.	w.	w. & s.w.	A gentle breeze at most places.
19	s.w. & s.	s.w.	s.w.	s.w. & w.	variable.	s.w.	variable.	variable.	Air in gentle motion. Rain to the N. [of Beattock, calm with fog.
20	s.w.	s.w.	s.w.	variable.	variable.	s.w.	s.w. & w.	variable.	A strong breeze at most places S. of Beattock. At every place N.
22	n.w.	n.	w.	variable.	n.	n.w.	n.w.	n.	A strong breeze at many places. Frosty at Lancaster.
23	e.	n.w.	n.	n.e.	variable.	variable.	n.w.	variable.	Very variable in strength.
24	n.	n.w.	n.	variable.	e.	s.e.	variable.	variable.	Nearly a calm day. Frosty at Shap.
25	s.e.	s.e.	s.	s.e.	s.e.	s.e.	s.e.	s.e. & e.	Air in gentle motion. Rain general.
26	n.e.	n.e.	n.	n.e.		n.e.	n.e.	s.e. & e.	A strong breeze at a few places in the South.
27	n.e.	n.e.	n.e.	n.e. & e.	s.e.	n.e.	variable.	variable.	A strong breeze to the South. A hard wind at Peterborough.
29	n.e.	n.e.	n.	variable.	n.e.	n.e.	variable.	variable.	Air in gentle motion.
30	n.e.	n.e.	n.	n.e.	e.	n.e.	n.e.	n. & n.e.	Wind variable in strength.

May 1850.	Direction of the Wind.								General Remarks.
	On the south coast.	On the south-east coast.	On the east coast.	On the north-east coast.	On the north-west coast.	On the south-west coast.	In the southern counties.	In the midland counties.	In the northern counties.
1	n.		n.	variable.	n.e.	n.e.	n.e.	n.e.	n.e. & e.
2	n.e.		n.e.	variable.	e.	n.e.	n.e.	n.	variable.
3	n. & n.w.	w.	w.	n.w. & w.	n.	s.	n.w. & n.	w.	n.w.
4	n. & n.w.	w.	n.w.	variable.	n.w.	n.w.	s.w. & w.	n.w. & w.	n.w.
5	s.w.	s.w.			w.				
6	n.e.	n.e.	e.	variable.	n.	n.e.	n.e.	e.	n.
7	n.e.	e.	s.e.	variable.	n.	n.e.	variable.	n.e.	s.e.
8	n.w.	s.	s.e.	variable.	n.	n.e.	n. & n.e.	variable.	variable.
9	n.	s.	s.w.	n.	e.	e.	s. & n.e.	variable.	variable.
10	s.w.	s.	s.w.	s.w.	s.w. & s.	s.w.	s. & s.w.	s. & s.w.	variable.
11	s.s.w.	s.	s.w.	w.	variable.	w.	s.w.	s.w.	variable.
12	n.	n.w.	n.	variable.	n.	variable.	n. & n.w.	n.w.	n. & w.
13	n.	n.w.	n.w.	n.w.	n.n.w.	n.w.	n.w. & w.	n.w.	n.w. & w.
14	n.e.	n.	n.	n.	n. & e.	n.e.	n.e.	n.e.	n.
15	n.e. & n.	n.	n.	n.w.	n.w.	n.e.	n.w.	n.w.	n.w. & w.
16	n.w. & n.		n.e.	variable.	variable.	n.w.	n.w.	n.w. & w.	variable.
17	n.	s.w.	n.e.	s.e.	e.		variable.	variable.	variable.
18	w. & s.w.	s.	s.w.	n. & e.	e.		n.e.	e.	variable.
19	e.	n.e.	n.	variable.	n. & e.	n.e.	n.e.	e.	s.e.
20	n.e.	n.e.	n.	variable.	variable.	s.w.	n.e.	variable.	variable.
21	e. & n.e.	s.	e.	n.e.	variable.	n.e.	n.e.	variable.	variable.
22	s.w.	s.w.	e.	s.e.	variable.	s.	s. & s.w.	e.	s.e.
23	n.e.	n.e.	e.	n. & n.e.	n.e.	n.e.	s. & s.w.	e.	variable.
24	w.	s.w.	s.w.	variable.	s.w.	s.w.	s.w. & s.	s.w.	s.e.
25	s.w. & s.	s.w.	s.w.	s.w. & w.	s.	s.w.	s.w. & s.	s.	s.
26	s.w.	s.w.	s.w.	w.	s.	s.w.	s.w. & s.	s.w.	s.
27	s.e.	n.e.	w.	s. & s.w.	s.	s.	s.w. & s.	s.w.	s.
28	n.e.		n.e.	s.e.	s.e.		variable.	variable.	variable.
29	n.w.		n.e.	s.e.	s.e.		variable.	variable.	variable.
30	n.w.		n.e.	s.e.	s.e.		variable.	variable.	variable.
31	n.w.		n.e.	s.e.	s.e.		variable.	variable.	variable.

June 1850.	Direction of the Wind.								General Remarks.
	On the south coast.	On the east coast.	On the north-east coast.	On the north-west coast.	On the south-west coast.	In the southern counties.	In the midland counties.	In the northern counties.	
1	n.e. & e.	n.e.	n.e.	s.e.	s.w.	s. & s.w.	s.e.	s.	Air in gentle motion. Fog at several places in the North.
2	e.			s.w.	s. & s.w.	s. & s.e.	s.e.	s.	A calm day. Fog to the North.
3	e. & n.e.	n.e.	n.e.	s.w. & w.	variable.	variable.	s. & s.e.	variable.	A calm day. Fog to the North.
4	e.	e.	n.e.	s.s.w.	s.	e. & s.e.	s.	s. & w.	Calm. Fog to the North.
5	variable.	s. & s.e.	s.w.	s. & s.e.	variable.	e. & s.	s. & s.w.	s. & w.	A calm day. Thunder at Dundee. Fog to the South.
6	s.	s.	s.w.	s.w.	s.w.	s.	s. & s.e.	s.	A calm day. Thunder at Dundee. Fog to the South.
7	s.w.	s.w.	s.w.	w.	s.w.	s.w.	s.w. & w.	s.w. & s.	A strong breeze to the South. Nearly calm in the North.
8	w. & s.w.	w.	s.w.	w. & n.w.	variable.	variable.	n.w. & w.	n.w. & w.	A strong breeze at most places. Rain general N. of Liverpool.
9	w. & s.w.	s.w.	w.	s.w.	s.w.	variable.	n.w. & w.	s. & w.	A strong breeze at most places.
10	variable.	n.e.	s.e.	s.w. & w.	s.e. & s.	variable.	s.w.	s. & s.w.	Air in gentle motion.
11	n.e.	s.w.	s.w.	s.w. & w.	n.e. & s.w.	w. & s.w.	s.	s. & w.	Calm to the South. Strong breeze and rain to the North.
12	s.w.	w.	w.	s.s.w.	s.w.	s.w.	s.w.	s. & w.	Air in gentle motion.
13	w.n.w.	w.	s.w.	w.n.w.	s.w.	s.w.	variable.	s.s.w.	A strong breeze at most places. Hard wind and rain in the N.
14	w.s.w.	w.	s.w.	variable.	variable.	w.s.w.	w.	variable.	A hard wind general. Gale at Lanark. Hail at Tamworth.
15	w.n.w.	w.	n.e.	variable.	variable.	s.	s.	variable.	Air in gentle motion. Rain general to the South.
16	s.s.e.		n.e.	n. & w.	n.w.	n.n.w.	n.w.	variable.	Air in gentle motion. Hard wind to the North.
17	variable.	s.e.		s.e.	w.s.w.	s.			A calm day.
18	variable.	variable.	s.	variable.	variable.	variable.	s.w.	s.	Fog at Sunderland.
19	variable.	w.s.w.	w.	variable.	s.w.	w.	variable.	variable.	Calm at many places.
20	variable.	w.	e.	variable.	s.w.	w.	variable.	variable.	A calm day.
21	s.s.w.	s.	s.w.	s.s.w.	n.w.	variable.	s.s.w.	variable.	Air in gentle motion.
22	variable.	s.w.	w.	s.	n.w.	w.	s.s.w.	variable.	Air in gentle motion.
24	s.s.e.	e.	s.	w.s.w.	n.w.	w.	s.s.w.	variable.	A calm day.
25	variable.	n.e.	e.	variable.	variable.	variable.	variable.	s.s.w.	Air in gentle motion.
26	s.e.	n.e.	n.e.	n.n.e.	n.e.	e.n.e.	e.n.e.	variable.	Light airs and calm prevalent.
27	n.e.	n.e.	n.e.	n.n.e.	variable.	e.	variable.	variable.	Strong breeze to the North, with rain.
28	n.n.e.	e.	e.	variable.		e.	e.n.e.	variable.	A calm day. Sultry at Crewe.
29	n.w.	n.w.	w.	s.s.w.		e.	variable.	variable.	Air in gentle motion.
						variable.			A thunder-storm at Plymouth.
									Wind variable in strength.

The mean monthly values of the several subjects of investigation during the past quarter are published in the Quarterly Report of the Registrar-General. Their quarterly values are shown in the following table :—

The mean of the numbers in the first column is $29\cdot561$ inches, and it represents that portion of the reading of the barometer due to the pressure of air; the remaining portion, or that due to the pressure of water, is $0\cdot322$ inch; the sum of those two numbers is $29\cdot883$ inches, and it represents the mean reading of the barometer for the quarter ending June 30, 1850.

The mean of the numbers in the second column for Guernsey, and those places situated in the counties of Cornwall and Devonshire, is $53^{\circ}5$; at Liverpool and Whitehaven is $51^{\circ}6$; for those places situated south of latitude of 52° is 53° ; for those places situated between the latitudes of 52° and 53° is 52° ; between latitudes 53° and 54° is $51^{\circ}0$; and for Durham, North Shields and Newcastle is $49^{\circ}3$. The fall of rain was greatest in Cornwall and Devonshire, averaging $8\cdot3$ inches; and it was the least between the latitudes of 52° and 53° , averaging $4\cdot8$ inches.

The highest reading of the thermometer in air was 87° at Uckfield and Nottingham; and *the lowest readings* were 25° at Uckfield and 26° at Wakefield. The extreme range of temperature during the quarter in England was therefore about 60° .

The least daily ranges of temperature took place at Guernsey, Liverpool and North Shields; their mean value was $10^{\circ}4$; and the greatest occurred at Uckfield, Aylesbury and Hartwell; their mean value was $21^{\circ}3$.

The least monthly ranges of temperature occurred at Guernsey, Torquay and Liverpool; their mean value was $25^{\circ}1$. The greatest took place at Uckfield, Aylesbury and Nottingham, and their mean value was $45^{\circ}8$.

Rain fell on the least number of days at Helston, Holkham, Norwich and Newcastle; the average number at these places was 53. It fell on the greatest number of days at North Shields, Wakefield and Derby. The average number at these places was 56. The places at which the largest falls took place were Southampton, Stonyhurst and Exeter; and the average amount at these places was $9\cdot5$ inches. The smallest falls occurred at Hartwell, Holkham and Liverpool; and their average was 4 inches.

Wheat in ear, on June 9 at Aylesbury; on the 10th at Leinslade and Hawarden; on the 11th at Holkham; on the 12th at Cardington; on the 16th at Helston, Stone, Hartwell

Meteorological Table for the Quarter ending June 30, 1850.

The observations have been reduced to mean values, and the hygrometrical results have been deduced from Glaisher's Tables.

Names of the places.	Mean pressure of air reduced to the level of the sea.	Mean temperature of the air.	Highest reading of the thermometer.	Lowest reading of the thermometer.	Mean daily range of temperature.	Mean monthly range.	Range of temperature in the quarter.	Mean temperature of the dew-point.	Wind.		Mean amount of cloud.	Rain.		Mean weight of vapour poured in a cubic foot of air.	Mean additional weight of vapour required to saturate a cubic foot of air.	Mean degree of humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Height of column of the barometer above the level of the sea.
									Strength.	General direction.		Number of days on which it fell.	Amount collected.						
Guernsey	in. 29.575	52.9	75.0	41.0	10.2	22.6	34.0	48.3	1.5	w.	4.5	42	7.2	4.1	0.6	0.854	4.9	535	123
Helston	29.455	52.2	86.0	32.0	16.1	35.3	54.0	48.6	1.6	s.w. & n.w.	5.4	34	8.4	3.9	1.4	0.747	5.0	531	106
Falmouth		52.0	84.0	34.0	16.4	35.0	50.0		1.4	Variable.	6.5	41	8.7						120
Torquay		54.1	75.0	40.0	11.6	26.0	35.0	46.8	1.4	w. & n.e.	6.0	42	8.8	4.5	1.1	0.758	4.6	535	
Turco		52.6	83.0	28.0	13.6	37.3	55.0	46.6	1.1	n. & s.w.	3.7	38	7.0	3.8	1.0	0.800	4.7	536	
Exeter	29.542	53.8	86.2	31.6	19.8	40.0	54.6	47.5	1.8	Variable.	3.7	43	9.0	4.0	0.9	0.813	4.7	533	140
Chichester		52.3	81.0	33.0	16.6	37.3	48.0			Variable.			7.7						
Uckfield		53.8	87.0	25.0	21.5	48.3	62.0	47.7		s.w.		38	8.1	3.7	1.4	0.704	4.5	532	180
Southampton	29.378	53.5	82.0	30.3	20.3	38.0	51.7	49.3	0.4		6.0	41	10.1	4.2	1.1	0.799	5.0	537	55
Royal Observatory, Greenwich	29.574	53.5	85.1	31.7	17.5	38.5	50.8	45.1		Variable.	6.6	45	5.6	3.9	1.2	0.754	4.4	532	159
Maidenstone Hill, Greenwich	29.582	52.7	81.3	30.5	18.1	38.5	50.8	48.5			6.4	44	5.7	3.3	0.8	0.847	5.0	533	107
St. John's Wood	29.591	53.0	81.7	32.0	18.1	40.5	49.7	42.4	1.6			44	5.4	3.8	1.4	0.712	4.0	533	150
Chiswell Street, London	29.595	55.6	77.0	40.0	12.1	29.3	37.0	46.9				40	5.6	3.8	1.4	0.734	4.7	530	284
Aylesbury	29.663	53.2	86.0	30.0	21.0	41.0	56.0	43.8	0.6	s. & w.	7.3	41	5.1	3.5	1.4	0.719	4.2	530	284
Stone Observatory	29.663	51.9	82.5	29.9	19.5	41.3	52.6	44.4	0.9	s.w. & n.e.		43	5.4	3.5	1.0	0.777	4.3	529	320
Hartwell (near Aylesbury)	29.558	52.6	85.0	29.0	21.4	43.1	56.0	45.3	1.2	s.w. & n.e.	7.0	47	4.0	3.7	1.1	0.773	4.4	532	250
Hartwell Rectory	29.570	51.9	84.2	31.0	19.2	40.6	53.2	44.1	0.8	s.w. & n.w.	5.8	46	3.9	3.5	1.1	0.762	4.4	530	290
Leinslade (Bucks)		52.9	85.0	32.0	18.7	40.7	53.0			n.e.		47	4.8						313
Radcliffe Observatory, Oxford	29.577	52.0						47.3	2.1	n.e. & s.w.	7.3	40	6.3	3.9	0.7	0.850	4.7	531	210
Rose Hill, Oxford	29.587	52.5	86.0	30.9	19.1	41.9	55.1	47.3	2.0	s.w.	7.5	45		4.0	1.0	0.834	4.9	530	270
Cardington	29.567	52.5	83.0	30.8	19.2	41.6	52.2	45.6	1.2	s. & s.w.	7.0	39	4.9	3.7	1.0	0.778	4.5	534	100
Norwich	29.518	51.2	80.0	30.0	16.3	38.6	50.0	45.9		n.e. & s.w.	7.0	30	5.2	3.7	0.8	0.840	4.5	530	23
Holkham	29.583	51.3	81.2	30.3	15.7	42.0	50.9	43.9	1.2	n.e.	6.3	36	4.1	3.5	1.0	0.785	4.2	537	39
Nottingham	29.518	52.4	87.2	29.3	20.8	45.0	57.4	45.5	0.6	n.e.	7.0	50	4.9	3.7	1.0	0.789	4.5	532	103
Derby		50.0	80.0	29.0	17.8	37.7	51.0	46.7			6.6	51	5.2	3.8	0.7	0.848	4.6	535	
Manchester	29.569	52.6						45.8		s.w.		46	5.6	3.8	1.0	0.790	4.6	533	144
Liverpool	29.550	51.1	75.5	32.4	13.5	33.5	43.1	43.7	2.0	s.	7.0	38	5.1	3.5	1.0	0.782	4.9	531	260
Hawarden	29.536	52.9	74.7	36.7	10.6	26.7	38.0	46.7	1.2	Variable.	6.3	38	4.2	3.9	0.9	0.819	4.7	536	37
Wakfield	29.550	50.9	84.0	26.0	20.0	42.7	58.0	44.2	1.9	Variable.	7.2	55	5.0	3.6	1.0	0.744	4.2	535	115
Leeds	29.597	50.5	80.0	30.0	17.6	39.0	50.0	44.3	1.4		5.0	46	5.1	3.5	0.8	0.820	4.3	531	125
Stonyhurst	29.578	49.6	79.0	27.9	16.7	36.9	51.1	42.3	1.3	w.	7.7	47	9.5	3.5	0.6	0.799	4.3	530	381
York	29.562	50.4	77.0	31.0	15.3	33.7	46.0	44.4		n.e. & s.w.		42	4.5	3.6	0.9	0.793	4.3	536	50
Whitehaven	29.531	50.3	78.5	32.5	12.4	33.8	46.3	44.8	1.9	s.w.	6.6	43	6.4	3.6	0.6	0.860	4.3	537	90
Durham	29.424	49.1	76.8	27.1	14.1	39.2	49.7	44.8	0.9	Variable.		46	4.8	3.6	0.7	0.839	4.4	531	340
Newcastle		50.6	80.0	32.0	12.9	32.3	48.0	44.4		s.e. & n.w.	6.0	33		3.6	0.9	0.804	4.3	536	
North Shields	29.584	48.1	73.7	33.0	10.4	29.5	40.7	44.6		Variable.		61	5.5	3.6	0.4	0.934	4.3	510	90
Glasgow		49.7									11	12	13	14	15	16	17	18	19
Number of columns	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19

House, Hartwell Rectory and Oxford; on the 20th at Nottingham; and on the 24th at Leeds.

Wheat in flower, on June 8 at Jersey; on the 10th at Uckfield; at Guernsey on the 15th; at Holkham and Stonyhurst on the 20th; at Stone on the 21st; at Hawarden on the 22nd; the white at Wakefield on the 22nd; the red, in the same field, on the 26th; on the 23rd at Helston, Hartwell, Leinslade and Derby; the 24th at Hartwell Rectory; the 25th at Cardington; the 26th at Nottingham; on the 28th at Rose Hill near Oxford; and on the 30th at Leeds.

Hay begun to be gathered, at Hartwell Rectory and Stone on the 18th of June; on the 24th at Hawarden and Whitehaven; and on the 27th at Durham.

The common lilac in flower, on April 22 at Jersey; on April 25 at Guernsey; at Helston on April 27; at Uckfield on May 5; on the 10th at Hartwell House and Wakefield; on the 11th at Aylesbury; on the 12th at Hartwell Rectory, Radcliffe Observatory, Oxford; on the 13th at Rose Hill near Oxford; at Stone on the 16th; at Hawarden on the 16th; on the 19th at Nottingham; on the 20th at Cardington; on the 22nd at Leeds; on the 23rd at Derby and Holkham; on the 27th at Stonyhurst; and on the 30th at Durham.

The cuckoo was first heard, on April 11 at Uckfield; on the 12th at Stone; on the 16th at Whitehaven; and on the 21st at Hartwell.

The nightingale was first heard, on April 7 at Uckfield; at Hartwell Rectory on the 12th; and at Stone on the 16th.

The first swallow seen, on April 3 at Stone; on the 18th at Whitehaven; on the 22nd at Hartwell Rectory; and May 21 at Durham.

At Rose Hill, Oxford, as observed by the Rev. John Slatter, F.R.A.S., the swallows on May 6 were packed together in a mass under the lee of a chimney, after flying about in vain for food, and even searching for flies by creeping amongst the weeds in a garden, and turning up the leaves as sparrows.

General remarks and agricultural reports.—At Exeter April was an unusually wet month, the amount of rain being more than double the average for April here. The mean temperature however was about 3° above the average.

The general character of May, as compared with those of former years, was rather cold and wet. The amount of rain was about $\frac{1}{2}$ an inch above, and the temperature was about 2° below the average.

The mean temperature and the total amount of rain during June corresponded very nearly with the average of the preceding twenty years.

Aylesbury. J. Dell, Esq., F.R.A.S.

From the small quantity of rain that has fallen during the last six months, much inconvenience is beginning to be felt from the short supply of water. The large reservoir, which usually contains a sufficient quantity of water to supply the county jail during the summer months, has now only 5 feet in depth, although the usual allowance has been considerably reduced.

Agricultural report for the quarter ending June 30, 1850:—
Uckfield. From C. L. Prince, Esq.

During the first three weeks the weather was for the most part stormy, with a considerable fall of rain; but the temperature was more equable than is usual at this season of the year, there being a total absence of frost during this period. March having been unusually dry and favourable for agricultural labour, the heavy rains of April have been very beneficial to the grain crops. There were slight frosts on several mornings during the last week, but not severe enough materially to injure the progress of vegetation. After the first week in May the weather was seasonable; the warm growing showers which fell at intervals hastened vegetation considerably, and proved very beneficial to all the crops situated on the dry soils in this neighbourhood. A severe frost occurred on the 3rd of May, which in many situations almost entirely destroyed the forward blossom of the cherry, pear, plum, gooseberry, &c. Ice was observed an eighth of an inch in thickness, and the reading of a self-registering thermometer placed on grass was 21°. The temperature during the first three weeks of June was very variable, and the thermometer in the shade ranged from 32° to 84°; the extreme range of the month however was 70°, viz. 30° in the morning of the 16th, and 100° in the sun on the 26th. The low temperature just noticed is very unusual for the season; and had not the fruit been protected by the foliage, it must have been very much injured. Ice was observed early in the morning, and there was a hoar-frost on good radiating surfaces.

Stone. The Rev. J. B. Reade, F.R.S., says, "The effect of the frost on June 16 on the clay land in the Vale of Aylesbury was severely felt. The potatoes were greatly injured, and in some places the kidney beans were completely destroyed."

Hartwell House. Dr. Lee says, "that on June 16 the potatoe tops were much frozen."

Hartwell Rectory. The Rev. C. Lowndes, F.R.A.S.

On the night of June 15 the frost was so severe as to damage the potatoe crops very severely on the low grounds.

Nottingham. E. G. Lowe, Esq., F.R.A.S.

The following are the present appearances of the crops in this neighbourhood :— Wheat fully an average, and looks very promising.

Oats and barley will be a poor crop on hot sandy land, and very short in the straw. On cold clay lands it is likely to be good. On the whole they will be both below an average, as the dry hot weather in June, which was very favourable to wheat, was quite the reverse to oats and barley.

The hay crop will be much below an average. Much has been cut, but the recent rain has prevented half of this from being as yet housed.

Beans and peas are but little grown about here; but where they can grow, they are spoken favourably of.

Turnips and mangelwurzel are good in some localities and bad in others.

Clover has not been an average.

Amongst fruit, currants and gooseberries are exceedingly abundant, especially the latter; and there are plenty of strawberries. Apples, pears and plums will be very deficient, although in some places the latter are abundant. Apricots, peaches and nectarines are very deficient, though the former has turned out the best crop of the three.

Hawarden near Chester. From Dr. Moffatt.

From early in March to the beginning of June, oats, barley, potatoes, turnips, &c. were put into the ground in their season; the seed time was a very favourable one, and the land was in good condition for its cultivation. From the extreme cold which prevailed at the end of March and beginning of April, and the want of rain from the middle of April to the second week in May, grass was scanty, and cattle were not turned out so early as usual; but the rains of the end of May and beginning of June were just in time to retrieve the coming crop from its wavering condition.

Potatoes have been planted to a greater extent in this than in former years; and if we may judge of their condition from the appearance of the stalk, &c., they are healthy. There are rumours however of "the disease." I find that everybody has heard of a reappearance of the disease, but it has not been seen by any one in this immediate locality.

Turnips look well, and I have not heard of the usual complaints of "fly" and "worm." Disease has been very common among cattle since the end of last March, and many farmers have had serious losses; indeed, one informed me, the other day, that since the 16th of that month he had lost ten cows from disease of the lungs.

From the first week in May to the second week in June an epidemic affection of the throat prevailed among horses. It consisted chiefly of swelling of the throat, with slight fever, which continued for a few days, and terminated with profuse discharge of thick mucus through the nose. I am not aware of its having been fatal in any case. I may sum up and conclude by saying, that we have had a very favourable "seed time," that the land generally speaking has been well prepared, and the weather favourable for cultivation; that the crops look healthy, and that there is every prospect of an early and an abundant harvest.

Rose Hill, Oxford.

The crops of corn and potatoes are healthy and promising. Stonyhurst. The Rev. A. Weld, F.R.A.S.

The cold east winds in April kept the grass back to such a degree, and caused so great a deficiency in the pastures, as to give but a very poor prospect of a good hay harvest, and induced many farmers to sow a greater amount of grain crops than usual. Last year's hay is not to be had in the country. The wet weather which prevailed during the early part of April caused a great delay in the planting of grain crops. Oats were first seen about April 10; clover about April 23, but was not all sown till May 1. Potatoes were not got in till May 2. All these different crops are now looking exceedingly well. The crop of beans has been unusually good all over the country. Turnips have been a good deal infected with the fly; so much so that some farmers had to sow a second time. Sheep were washed about May 31, shorn on June 10. The late rains have brought forward the grass crops with extraordinary rapidity; they are now heavy, and farmers are waiting only for fine weather to mow. Clover has been housed, but was light. With this exception, a little rye-grass, nothing has yet been cut.

West Riding of Yorkshire. Charles Charnock, Esq.

The quarter now passed has been generally very cold and dry, and dry soils show in many places a want of rain; pastures are bare, and meadows are generally light. The season is late, both wheat and barley shooting about ten days later than in an average season.

Potatoes are planted very largely, full one-third more than usual. I have seen a few instances of the disease, but it is very rare; this however is rather early to pronounce the danger passed for the season. Of turnips a small breadth only has been sown this year, partly from the great breadth of potatoes, and partly from many farmers finding their sheep stock at the present prices (4d. per lb.) are unprofitable. Bar-

ley is very much varied ; on good land in good cultivation the crop is good, but on dry soils it is light and thin. A very small breadth of barley is sown this season. Wheat is very varied, the crop being from very bad to good according to the soils and management, the dry season having seriously affected the dry soils.

An unusually great breadth of spring wheat has been sown, displacing, as above stated, a portion of the barley crop.

Stock has generally done well where healthy, but Pneumonia has been very prevalent and fatal in some districts, generally among Irish cattle. Potatoes, from the dry cold weather, are later than usual.

XV. Notices respecting New Books.

The Course of Creation. By JOHN ANDERSON, D.D.
London : Longman and Co.

WE have been much gratified by the perusal of this work, not because at the present day physical science, and more especially the most poetical of all the branches of it, Geology, requires any defence from the reproach of being hostile to religion, but because the plain and simple yet forcible and eloquent manner in which the records graven upon the stone book of nature are set forth, have invested this production with unusual interest ; and the more so, as emanating from the pen of a divine fully imbued with the important bearings which exist between natural science and revealed truth. "The Course of Creation" is no philosophic theory—involves no metaphysical inquiry into the origin of things—nor affords any new facts connected with the science itself ; but is simply intended to express the feelings engendered in the mind of the author by geological pursuits ; and thus it aims to impress others with a desire to become acquainted with the works of the Creator and the records of His will.

The subject-matter of the volume consists of the various geological phænomena to be met with in the line of country occurring between the Grampians and the Alps ; and the relations of the several geological formations to each other are treated of in geographical sequence.

Thus we have the general structure of Scotland first mentioned, with its series of crystalline, trap and palæozoic rocks, including descriptions of the more important animal and vegetable remains found in the latter ; to this succeeds the geological structure of England with its peculiar and interesting features, from the Silurian to the close of the tertiary period ; the concluding geological portion is devoted to France and Switzerland, in which we find a concise account of the tertiary basin of Paris and the Loire, the volcanic district of Auvergne, and the structure of the Alps, &c.

The descriptions are embodied not only from personal observation, but enriched by an extensive acquaintance with, and careful examination of, the most recent geological works.

We only notice one misstatement of any importance which appears *Phil. Mag.* S. 3. Vol. 37. No. 248. August 1850. L

to have escaped the author's attention: in speaking of the Swiss Alps, at p. 295 it is stated, that "the Oxfordian group are represented by the 'Neocomian' limestones, a series of strata abounding in fossils of the gault and upper greensand." According to Sir R. Murchison, Von Buch, and other geologists, the occurrence of the Oxfordian group with its characteristic shells is distinctly traced in the French and Swiss Alps, and forms the base of all the outer edges of the Savoy Alps, overlaid by the Neocomian limestones, which are again surmounted in the Savoy Alps by a dark-coloured limestone, which, from its contained fossils, fairly represents the gault and upper greensand.

The concluding part of the work is devoted to general principles, in which we find the theories of organic life discussed, especially as bearing on the development hypothesis,—the causes of extinction of the various forms of animated life which have successively tenanted our globe,—the analogical order of moral and physical progression,—as well as a chapter on the Mosaic account of creation as reconcilable with geological discoveries; want of space prevents us from extracting the many valuable and eloquent passages contained in this portion, involving points of the highest interest.

To those who have not hitherto investigated the varied and multifiform changes, both organic and inorganic, to which the superficial crust of our globe has been subjected, or have regarded geology as an unprofitable and uninviting study, and to the geologist himself occupied merely with the laborious task of collecting the dry statical details connected with his science, we cordially recommend the perusal of this volume, as unfolding in the plainest manner the beauty, harmony and beneficence of Creative power that has reigned throughout all past time, and by which the present surface of our earth has been elaborated, modified, and adapted to the wants of an intellectual and moral being, pointing out to us at the same time, that as an inquiry "into the records of Creation, geology has disclosed views, and elicited discoveries, of the works of the Divine Architect of the world which the religious inquirer will as cordially embrace, as ignorance can overlook or misapprehend."

XVI. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 69.]

Feb. 25, **O**N the Symbols of Logic, the theory of the Syllogism, 1850. and in particular of the Copula, and the application of the Theory of Probabilities to some questions of evidence. By Professor De Morgan.

This paper, which is in continuation of the one published in vol. viii. part 3 (read Nov. 9, 1846), and of subsequent additions contained in the author's work on *Formal Logic*, is divided into six sections.

Section I. *On the approximation of logical and algebraical modes of thought.*—The subjects of this section are,—1st, some development of the idea that the oppositions of logic have affinities which may

one day lead to a connected theory, making use of a common instrument, just as the oppositions of quantity which are considered in algebra are connected by the general theory of the signs + and -; and 2nd, some remarks on the resemblance of the instrumental part of inference to algebraic elimination.

Ten such instances as affirmative and negative, conclusive and inconclusive, &c., are compared with the logical distinction of *universal* and *particular*; and it is pointed out, in all the cases in which it is not already acknowledged, that it would be *possible* to use any one of the ten in place of the last.

Section II. *On the formation of symbolic notation for propositions and syllogisms.*—Exclusive of remarks on the Aristotelian notation and on notation in general, and a statement for comparison of Sir William Hamilton's notation, this section contains the following matters.

1. A pictorial or diagrammatic representation of syllogistic inferences, being after the method pursued by Lambert, with such additions as will enable the system to represent all the cases in which contraries are used.

2. An abbreviated and arbitrary method of representing propositions and syllogisms.

Following Sir William Hamilton in making the quantity of both subject and predicate matter of symbolic expression, Mr. De Morgan gives his system of notation two new features. First, he dispenses with the representatives of the terms (except when it may be convenient to introduce them for the time), and represents the proposition by the *symbols of quantity only*, and the presence or absence of a sign of negation. Secondly, instead of making the symbols of universal and particular absolute, he gives one symbol, $\cup$, to a universal subject and a particular predicate, and another, $\cap$, to a particular subject and a universal predicate: a dot $\cdot$ signifying negation. Thus $X\cup(Y$, or simply $\cup\cap$, represents 'No X is Y': $X\cap(Y$, or $\cap\cap$, represents 'Some Xs are not any Ys': $X\cup)Y$ represents 'Some Xs are Ys.' Of the second circumstance above mentioned, Mr. De Morgan believes that it makes the rules easier, and knows that it makes the notation more suggestive.

Retaining in mind the order XY, YZ, XZ, which is the only figure used in the classification (being the first with inverted order of premises), the syllogism is to be denoted by the junction of the propositional symbols. Thus $\cup\cup\cup\cup=\cup\cup$ denotes 'Every X is Y, every Y is Z, therefore every X is Z.' When this is to be read in any figure, the subject-quantities are to have their symbols thickened, the second premise being read first: thus in the four figures, in order, will be seen such symbols as $\cup\cup, \cup\cup, \cup\cup, \cup\cup$.

Section III. *On the symbolic forms of the extension of the Aristotelian system in which contraries are introduced.*—This system is the one which was completed and published to the Society before any correspondence with Sir William Hamilton. Mr. De Morgan remarks that it contains (incidentally, not designedly) every distribution of quantifications; and gives his reasons for not dwelling on this fact while the controversy was unfinished, with his statement that it had not struck him when the controversy began. Mr. De Morgan

frequently distinguishes *this* system from Sir W. Hamilton's by calling the former that of *introduction of contraries*, the latter that of *invention of predicates*. For distinctness, it may be stated that Mr. De Morgan's other, or *numerically definite* system (the one concerned in the discussion), does not appear in the present paper, except as matter of allusion.

The forms of predication in this system are as follows, with reference to the order XY , x and y being not X and not Y .

Universals.

Affirmative	$\left\{ \begin{array}{l} A_1 \\ A^1 \end{array} \right.$	$\left\{ \begin{array}{l} X))Y \\ x))y \end{array} \right.$	or	$\left\{ \begin{array}{l} X((Y \\ X((y \end{array} \right.$	$\left\{ \begin{array}{l} \text{Every } X \text{ is } Y \\ \text{Every } Y \text{ is } X \end{array} \right.$
	$\left\{ \begin{array}{l} E_1 \\ E^1 \end{array} \right.$	$\left\{ \begin{array}{l} X))y \\ x))Y \end{array} \right.$	or	$\left\{ \begin{array}{l} X \cdot (Y \\ X \cdot (y \end{array} \right.$	$\left\{ \begin{array}{l} \text{No } X \text{ is } Y \\ \text{Everything is } X \text{ or } Y \text{ or both.} \end{array} \right.$

Particulars.

Affirmative	$\left\{ \begin{array}{l} I_1 \\ I^1 \end{array} \right.$	$\left\{ \begin{array}{l} X()Y \\ x()y \end{array} \right.$	or	$\left\{ \begin{array}{l} X()Y \\ X()y \end{array} \right.$	$\left\{ \begin{array}{l} \text{Some } X\text{s are } Y\text{s} \\ \text{Some things are neither } X\text{s nor } Y\text{s} \end{array} \right.$
	$\left\{ \begin{array}{l} O_1 \\ O^1 \end{array} \right.$	$\left\{ \begin{array}{l} X()y \\ x()Y \end{array} \right.$	or	$\left\{ \begin{array}{l} X \cdot (Y \\ X \cdot (y \end{array} \right.$	$\left\{ \begin{array}{l} \text{Some } X\text{s are not } Y\text{s} \\ \text{Some } Y\text{s are not } X\text{s.} \end{array} \right.$

Various rules of connexion are given, being all translations of those in the work on *Formal Logic*, except a classification of the particulars by *probability*, answering to that of universals. Thus of $X))Y$ and $X \cdot (Y$, each makes the other *impossible*: of their contraries $X \cdot (Y$ and $X()Y$, each, so far as it affects the other, reduces its probability.

It appears that a quantified term has a quantified contrary: that of 'Every X ' is 'some x s,' &c.

The symbolic *canon of validity* is;—if both middle parentheses turn the same way, there need be one universal proposition; if different ways, two. Thus $)))$ (and $(.))$ (both have inferences; and so has $).(())$; but $).(())$ has none. The symbolic *canon of inference* is;—erase all signs of the middle term, and what is left (two negations, if there, counting as an affirmation) shows the inference. Thus from $X \cdot (Y) \cdot (Z$ we infer $X \cdot ((Z$ or $X((Z$: more simply, from $(.))$ (we infer $(($.

Section IV. *On the symbolic forms of the system in which all the combinations of quantity are introduced by arbitrary invention of forms of predication* (Sir W. Hamilton's).

The modes of predication *peculiar* to this system have the same symbols, $)$ (and $(\cdot)$, as the peculiar propositions of the system of contraries; but with very different significations, as follows:—

Contraries.

$(\cdot)$ Universal negative with particular terms, and affirmative form in common language.

All things are either Xs or Ys.

$)$ (Particular affirmative with universal terms, and negative form in common language.

Some things are neither Xs nor Ys.

Invention of predicates.

$(\cdot)$ Particular negative with particular terms, not used in common language.

Some Xs are not some Ys.

$)$ (Universal affirmative with universal terms, being declaration of identity in common language.

All Xs are all Ys.

Mr. De Morgan argues that Sir William Hamilton's system cannot be called an *extension* of that of Aristotle, in the sense in which that word is used.

The forms of predication are as follows :—

$A_1 + A^1$	(All Xs are all Ys	E_1	·(No Xs are Ys
I_1	() Some Xs are some Ys	—	(· Some Xs are not some Ys
A_1	)) All Xs are some Ys	O^1	·) No Xs are some Ys
A^1	((Some Xs are all Ys	O_1	(·(Some Xs are no Ys.

Previously to entering upon the forms of syllogism, Mr. De Morgan repeats and reinforces the objections brought forward in his *Formal Logic*; namely, that $()$ (is a compound of $)$ and $(($, and has no simple contradiction in the system; and that $(\cdot)$ not only has no simple contradiction, but cannot be contradicted except when the terms are singular and identical. He then proceeds to propose one mode of remedying these defects. Calling the ordinary proposition *cumular*, he proposes to make it *exemplar*, as asserting or denying of one instance only. In the universal proposition, the example is *wholly indefinite, any one*; in the particular proposition it is *not wholly indefinite, some one*. The defects of contradiction are thus entirely removed, as in the following list, in which each universal proposition is followed by its contradiction.

$()$ (Any one X is any* one Y	$(($ (Some one X is any one Y
$(\cdot)$ (Some one X is not some one Y	$)\cdot$ (Any one X is not some one Y
$)$) Any one X is some one Y	$\cdot)$ (Any one X is not any one Y
$(\cdot)$ (Some one X is not any one Y	$()$ (Some one X is some one Y

In both systems there are thirty-six valid syllogisms, and in both the canon of validity is,—one universal (or wholly indefinite) middle term, and one affirmative proposition. But the symbolic canons of inference differ as follows (with reference to the order XY, YZ, XZ).

Exemplar system.—Erase the middle parentheses, and the symbol of the conclusion is left: thus $()\cdot$ gives $(\cdot)$.

Cumular system.—Erase the middle parentheses, and then, if both the erased parentheses turn the same way, turn any universal parenthesis which turns the other way, unless it be protected by a mark of negation. Thus $\cdot()$ gives $\cdot)$, $()$ gives $()$, and $()\cdot$ gives $(\cdot$.

Section V. *On the theory of the copula, and its connexion with the doctrine of figure*.—In his work on *Formal Logic*, Mr. De Morgan had analysed the copula, and abstracted what he calls the *copular conditions* of the relation connecting subject and predicate. These are, *transitiveness*, seen in such copulæ as *support*, *govern*, *is greater than*, &c., *ex. gr.* if A govern B, and B govern C, A governs C: and *convertibility*, seen in such copulæ as *is acquainted with*, *agrees with*, &c.; *ex. gr.* if A agree with B, B agrees with A. Mr. De Morgan's position is, that any mode of relation which satisfies both these conditions has as much claim to be the copula as the usual one, *is*, which derives its fitness entirely from satisfying the above conditions. So far the work cited. In the present paper the *correlative* copula is introduced, as *is supported* in opposition to *supports*, &c., and every system of syllogism is thus extended. If a copula be taken which is only trans-

* So that there can be but one X and one Y, and that X is Y.

itive, but not convertible, every syllogism remains valid, provided that the *correlative* of that copula be used instead of it, when needful. And in this consists, according to Mr. De Morgan, the root of the doctrine of *figure*. If + represent affirmative, and — negative, the four figures are connected with ++, +—, —+, and —— (in the system of contraries, where negative premises may have a valid conclusion, the fourth figure has equal claims with the rest, though the conditions of all the figures are singularly altered). These forms do not require the correlative copula: thus +— in the second figure (as *Camestres* and *Baroko* among the Aristotelian forms) are as valid when the copula is ‘*supports*’ or ‘*is greater than,*’ as when ‘*is*’ is employed. But in every other case the rule for the proper introduction of the correlative copula is as follows:—The preceding being called the *primitive forms* of the four figures, when *one* premise of a primitive form is altered, the necessity of a correlative copula is thrown upon *the other*; when *both*, upon the conclusion. Thus, the primitive form of the second figure being +—, and *Cesare* showing —+, it is only valid with the copula ‘*governs,*’ by making ‘*is not governed by*’ the copula of the conclusion, as follows:—

No Z governs any Y

Every X governs a Y

Therefore no X *is governed by* any Z.

By an additional letter (*g*) introduced into the usual words of syllogism, the places of the correlative copula may be remembered, as in *Barbara*, *Celagrent*, &c.: a *g* being made to accompany any member of the syllogism in which the correlative copula must be employed.

This theory is applied equally to the Aristotelian system, to Sir William Hamilton’s (though not of universal application in the *cumular* form), and to Mr. De Morgan’s system of contraries. The extensions required by the use of a merely transitive copula, in the last-mentioned system, are discussed; and mention is made of the *tricopular* system, in which the leading copula and its correlative have an intermediate or middle relation, equally connected with both; as in $> =$ and $<$ of the mathematicians.

The next step is the assertion that it is not necessary that any two of the three copulæ of a syllogism should be the same; all that is requisite is that, in affirmative syllogisms, the copular relation in the conclusion should be compounded of those in the premises. The instrumental part of inference is described by Mr. De Morgan as *the elimination of a term by composition (including resolution) of relations*, which leads to the conclusion that *whenever a negative premise occurs, there is a resolution of a compound relation*. This resolution is shown in a case (among others) of the ordinary copula, in which, however, it would hardly strike the mind more forcibly than would the properties of powers in algebra if every letter represented unity. Mr. De Morgan shows (in an addition) that in some isolated cases of inference which are not reducible to ordinary syllogism, logicians have had recourse to what amounts to composition of relations.

Mr. De Morgan next points out that the copular relation, in affirmative propositions, need not be restricted as applying to one

instance only of the predicate; and shows that the removal of this usual restriction entirely removes *all* his objections to Sir William Hamilton's form of his own system.

Section VI. *On the application of the theory of probabilities to some questions of evidence.*—This inquiry was suggested by the apparent (but only apparent) error of the logicians, who seem to lean towards the maxim that, when the subject and predicate are unknown, the universal and particular propositions 'Every X is Y,' 'Some Xs are not Ys,' are *à priori* of equal probability. The difficulty is one which occurs in the following case:—If a good witness, drawing a card from a pack, were to announce the seven of spades, his credit would not be lowered, though he would have asserted an event against which it was 51 to 1 *à priori*. A common person gives the true answer, 'Why not the seven of spades as well as any other?' Many readers of works on probability would be inclined to say 'That is not the question; why the seven of spades rather than some one or another of the fifty-one others?' The retort is fallacious: it rubs out the distinctive marks from the other fifty-one cards, and writes on each of them 'not the seven of spades' as its only exponent. Laplace has chosen two problems, in one of which the distinctive marks exist, and not in the other; and, neglecting the consideration of the first one, has founded his remarks upon the deterioration of evidence by the assertion of an improbable event, entirely upon the second. The object of this section is, by a closer examination of the mathematical problem of evidence, to ascertain the accordance or non-accordance of the results of usual data with usual notions. The result of the examination is, that common notions, as in other cases, are found closely accordant with theory. For instance, if there be n possible things which can happen, so that the *mean* probability of an event is $\frac{1}{n}$, a witness of whom we know no *particular bias* towards

one mode of error rather than another, asserting an event of which the *à priori* probability is a , has his previous credit raised, unaltered, or lowered, according as $a - \frac{1}{n}$ is positive, nothing, or negative. So

that though the *à priori* probabilities were distributed among a million of possible and distinguishable cases, yet a witness asserting one of them against which it is only 999,999 to 1, would have as good a right to be believed as though there had been but two equally probable cases, of which he had asserted one.

March 11.—Curvature of Imperfectly Elastic Beams. By Homersham Cox, B.A. Jesus College.

The equation to the curve of an elastic deflected beam is usually deduced from the assumption,—1, that the longitudinal compression or extension of an elastic filament is proportional to the compressing or extending force; 2, that for equal extension and compression the compressing and extending forces are equal to each other.

These hypotheses are not quite correct in practice. All substances appear to be subject to a *defect of elasticity*, *i. e.* their elastic forces of restitution increase in a somewhat less degree than in proportion to the extension or compression. If the forces be taken as functions

of the latter quantities expressed by a converging series of their ascending integral powers, the terms after the third may in general be neglected as of inconsiderable magnitude. If then e be the extension of a uniform rod of a unit of length and a unit of sectional area, the longitudinal force producing that extension is

$$\alpha e + \beta e^2 + \beta' e^3,$$

where α , β , β' are empirical constants.

Similarly, if c be the compression of a similar rod, the force producing that compression is

$$\gamma c + \delta c^2 + \delta' c^3,$$

where γ , δ , δ' are three other empirical constants.

These formulæ are to be applied to a uniform beam of rectangular section, resting on horizontal supports and slightly deflected at its centre. For this purpose, the compression and extension of every filament of the beam are expressed in terms of the radius of curvature and the distance from the neutral axis. Analytical expressions are thus obtained for the elastic forces developed in any transverse section of the beam; and the position of the neutral axis is obtained from the integrals of these expressions by the principle, that the sum of all the horizontal forces above is equal to the sum of all the horizontal forces below the neutral axis.

Next, the sums of the *moments* of the elastic forces about the neutral axis are obtained; and the sums are equated to the moment about that axis of the pressure (P) of the fulcrum, the latter moment being the product of half the deflecting weight by the distances (x) of the fulcrum from the point of the neutral axis here considered. This equation involves the radius of curvature, and is solved with respect to the reciprocal of that quantity. It is to be observed, that this equation, and also the preceding one determining the neutral axis, are not of such a form as to admit of direct solution, and are therefore solved by an ordinary method of approximation.

The reciprocal of the radius of curvature of a point (x , y) of a curve is equal to (the second differential of y with respect to x) $\div$ (a quantity which becomes equal to unity when, as here, the inclination to the axis of x of the tangent at any point of the curve is comparatively very small).

Making the substitution indicated, and integrating twice the equation last obtained, we obtained finally for the equation to the neutral line of a rectangular beam of vertical depth d , and horizontal breadth μ , and length $2a$,

$$y = \frac{\kappa x^3}{2 \cdot 3} - \frac{b \kappa^2 x^4}{3 \cdot 4} + \frac{(2b^2 - c) \kappa^3 x^5}{4 \cdot 5} - \left(\frac{\kappa a^2}{2} - \frac{b \kappa^2 a^3}{3} + \frac{(2b^2 - c) a^4 \kappa^3}{4} \right) x,$$

where

$$b = \frac{3}{4} d (\beta + \delta \alpha^2 \gamma^{-2}) (1 + \alpha^{\frac{1}{2}} \gamma^{-\frac{1}{2}})^{-2}$$

$$c = \frac{3}{5} \frac{d^3}{\alpha} (1 + \alpha^{\frac{1}{2}} \gamma^{-\frac{1}{2}})^{-3} (\beta' + \delta' \alpha^{\frac{5}{2}} \gamma^{-\frac{5}{2}})$$

$$\kappa = \frac{P}{\mu} \frac{3}{d^3 \alpha} (1 + \alpha^{\frac{1}{2}} \gamma^{-\frac{1}{2}})^2.$$

If, according to the ordinary hypotheses of perfect elasticity, we put $\alpha = \gamma$ and neglect terms depending on $\beta, \beta', \delta, \delta'$, this equation to the elastic curve coincides with that given by Poisson and others.

If we put $x = a$, the value of the deflection at the centre of the beam is

$$\frac{\alpha a^3}{3} - \frac{b\alpha^2 a^4}{4} + \frac{(2b^2 - c)\alpha^3 a^5}{5}.$$

Whence it may be seen that the deflection is greater than it would be if the elasticity were perfect.

XVII. *Intelligence and Miscellaneous Articles.*

CHEMICAL EXAMINATION OF A MINERAL CONTAINING OXIDE OF URANIUM, FROM THE NORTH SHORE OF LAKE SUPERIOR.
BY J. D. WHITNEY.

THE specimen of which the analysis follows, was given me by J. W. Foster, Esq., and is the same mineral which has been named Coracite by Mr. J. L. Le Conte, and partially described by him in the American Journal of Science (New Series, vol. iii. p. 174). As it is evident that the conclusions drawn by Mr. Le Conte from his qualitative examination were quite incorrect, and as the mineral differs considerably, in its reaction with acids, from pitchblende, with which it has the greatest analogy, and which at first sight it would seem to be, I have carefully examined it, with the following results :—

Substance amorphous; fracture uneven; without traces of cleavage; hardness 3; spec. grav. —; colour pitch-black; powder gray; lustre resinous.

Before the blowpipe it does not change its appearance, or fuse, or colour the flame. It gives with the fluxes the characteristic reactions of uranium.

It dissolves readily without the application of heat in dilute hydrochloric acid, effervescing strongly; in which respect it differs entirely from pitchblende, which is insoluble, except in nitric acid or in aqua regia. It gives a beautiful green solution, a small quantity of floccy silica separating.

The analysis was conducted as follows :—

A portion of the mineral, carefully selected and freed from foreign matters, was pulverized and dried at 100° C. It was then dissolved by hydrochloric acid in a suitable apparatus, the loss of weight being considered as carbonic acid. The silica separated by filtration was found to be pure when tested by the blowpipe, and was entirely soluble in carbonate of soda. In the solution filtered from the silica, sulphuretted hydrogen threw down a precipitate, at first dark brown and afterwards black, of sulphuret of lead, which was estimated as sulphate of lead by oxidizing with nitric acid. The filtered solution was then digested till it no longer smelt of sulphuretted hydrogen, and oxides of uranium and iron and alumina precipitated by caustic ammonia. The precipitate was washed with water to which chloride of ammonium had been added, and then taken moist from the

filter and redissolved in hydrochloric acid. In this solution oxide of iron and alumina were precipitated by carbonate of ammonia, the oxide of uranium remaining in solution, and care being taken that the solution should be quite dilute, in order that the iron might be entirely precipitated. The oxides of iron and alumina were separated by caustic potash. In the solution filtered from these substances, the uranium was precipitated by adding hydrochloric acid to supersaturation, boiling to expel all the carbonic acid, and then adding ammonia.

In the solution from which the precipitate by ammonia of uranium, iron and alumina had been separated, the lime was thrown down by ammonia and oxalic acid. The filtered solution was evaporated to dryness, and the ammoniacal salts driven off by ignition, when there remained traces of magnesia and manganese.

The water was estimated by ignition in a bulb-tube, and collecting the water driven off in a weighed chloride of calcium tube. The mineral does not however part with any of its carbonic acid at a temperature below that required to drive off all the water, nor is it rendered less soluble by exposure to the strongest heat of a Berzelius lamp.

No traces of sulphur could be found by boiling the mineral with fuming nitric acid, and testing with chloride of barium. The lead has therefore been calculated as oxide, and not as a sulphuret.

The per-centage results of two analyses are as follows:—

	I.	II.
Silica	4.35	5.60
Alumina.....	0.90	3.64
Oxide of iron.....	2.24	
Oxide of uranium	59.30	57.54
Oxide of lead.....	5.36	5.84
Lime	14.44	13.47
Carbonic acid.....	7.47	
Water.....	4.64	
Magnesia and manganese	traces	
	98.70	

That the uranium exists in the mineral as U^2O^3 , and not as $UO U^2O^3$, as in the common pitchblende, is evident from its ready solubility in acids; and that the oxide of uranium, or uranic acid as it might with equal propriety be called, is in chemical combination in the mineral is equally evident, from the fact that its solubility is not diminished by ignition. That the silica is also chemically combined is shown by the fact that it is separated in a state in which it is soluble in carbonate of soda. It is difficult to see in exactly what manner these elements are combined with regard to each other, though it is probable that the oxide of uranium plays the part of an acid toward a portion of the lime (the remaining portion being in combination with the carbonic acid) and the lead. The frequent occurrence of a small quantity of oxide of lead with the ores of uranium is an interesting fact, on which future investigations may perhaps throw some light,—*Boston Journal of Natural History*, vol. vi. p. 37.

ON THE DUST-STORMS OF INDIA. BY P. BADDELEY, ESQ.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Lahore, April 18, 1850.

I have only an hour or two to spare before the Indian mail leaves this, to give you a few notes regarding dust-storms, which are very prevalent in this part of India during the dry months of April, May and June, that is, before the setting in of the rainy season.

My observations on this subject have extended as far back as the hot weather of 1847, when I first came to Lahore, and the result is as follows:—Dust-storms are caused by spiral columns of the electric fluid passing from the atmosphere to the earth; they have an onward motion—a revolving motion, like revolving storms at sea—and a peculiar spiral motion from above downwards, like a corkscrew. It seems probable that in an extensive dust-storm there are many of these columns moving on together in the same direction; and during the continuance of the storm, many sudden gusts take place at intervals, during which time the electric tension is at its maximum. These storms hereabouts mostly commence from the north-west or west, and in the course of an hour, more or less, they have nearly completed the circle, and have passed onwards.

Precisely the same phenomena, in kind, are observable in all cases of dust-storms: from the one of a few inches in diameter to those that extend for fifty miles and upwards, the phenomena are identical.

It is a curious fact, that some of the smaller dust-storms occasionally seen in extensive and arid plains, both in the country and in Afghanistan above the Bolon Pass, called in familiar language “Devils,” are either stationary for a long time, that is, upwards of an hour, or nearly so; and during the whole of this time the dust and minute bodies on the ground are kept whirling about into the air. In other cases these small dust-storms are seen slowly advancing, and when numerous, usually proceed in the same direction. Birds, kites and vultures, are often seen soaring high up just above these spots, apparently following the direction of the column, as if enjoying it. My idea is, that the phenomena connected with dust-storms are identical with those present in waterspouts and white squalls at sea, and revolving storms and tornadoes of all kinds; and that they originate from the same cause, viz. moving columns of electricity.

In 1847, at Lahore, being desirous of ascertaining the nature of dust-storms, I projected into the air an insulated copper wire on a bamboo on the top of my house, and brought the wire into my room, and connected it with a gold-leaf electrometer and a detached wire communicating with the earth. A day or two after, during the passage of a small dust-storm, I had the pleasure of observing the electric fluid passing in vivid sparks from one wire to another, and of course strongly affecting the electrometer. The thing was now explained; and since then I have by the same means observed at least sixty dust-storms of various sizes, all presenting the same phenomena in kind,

I have commonly observed that, towards the close of a storm of this kind, a fall of rain suddenly takes place, and instantly the stream

of electricity ceases, or is much diminished; and when it continues, it seems only on occasions, when the storm is severe and continues for some time after. The barometer steadily rises throughout. In this part of the world, the fluctuation of the barometric column is very slight, seldom more than two or three-tenths of an inch at a time.

The average height at Lahore is 1·180, corrected for temperature, indicating, I suppose, above 1150 feet above the level of the sea, taking 30 inches as the standard.

A large dust-storm is usually preceded by certain peculiarities in the dew-point, and the manner in which the particles of dew are deposited on the bulb of a thermometer. My mode of taking the dew-point is, to plunge a common thermometer in a little ice, let it run down 20° or 30°, take it out, wipe it dry, hold it up to the light, and observe the bright spot, and continue to wipe off the dew so long as it is deposited and dulls the bulb: at the instant it clears off mark the temperature. This I have compared frequently with Daniell's hygrometer, cooled by means of chloroform, and find them both correspond with the greatest accuracy.

This is a digression; but I have no time to arrange, and must therefore put down my remarks as they occur to me.

The dew-point varies very much, but is usually many degrees below the temperature of air, 20° to 50° or more.

It also varies according to the time of year. During November last the mean temperature of dew-point was about 47°, that of the air about 71°.

In January 1850, dew-point 43°; in the air, 61°; and the mean temperature of self-registering thermometer 45°·4.

In February 1850, mean of dew-point 48°, and air 64°·5.

April 1850, mean temperature of dew-point so far is about 60°, and the air 84°.

The sparks or the stream of electricity, as it is seen passing from one wire to the other, is in some cases, and during high tension, doubled or trebled; and is never straight, but invariably more or less crooked.

Various kinds of sparks are seen at times; one end of the wire has a star; and from the wire, when held just beyond striking distance, a brush is seen curved, which, when viewed through a lens, seems composed of a stream or curved brush of bright globules, like a shower of mercury.

The manner in which the electricity acts upon the dust and light bodies it meets with in its passage, is simple enough. I suppose the particles similarly electrified and mutually repulsive, and then, together with the whirling motion communicated to them, are whisked into the air. The same takes place when the electricity moves over water. The surface of the water becomes exposed to the electric agency; and its particles, rendered mutually repulsive, are in the same way whirled into the air.

At sea the waterspout is thus formed. First of all is seen the cloud descending, and beneath may be observed the water in a cone, misty and agitated; soon the cloud is seen to approach and join the

latter, involving both extremities in one column, having a spiral motion, and on it moves or continues stationary. The power of electricity in raising bodies, when combined with this peculiar whirling motion, will account for fish, &c. being carried up in its vortex and afterwards discharged to a distance on the earth. The motion of the dust-storm may be described by spinning a tee-totum on a drop of ink; and the way in which bodies are projected may be in like manner described, by letting fall a drop of ink on the centre of a tee-totum while spinning. In this case the particles of ink are thrown off at tangents ever varying, as the centre moves; and perhaps it will be found, that when these kind of storms pass through forests, trees uprooted are distributed something in this manner.

The violent dust-storms are by some supposed to commence at the foot of the hills. I cannot tell if this be the case or not, but should think that they do not necessarily do so, as many often originate in extensive arid plains; and the rarefaction of air, from great and long-continued heat, may be in some way connected with the exciting cause.

Some of them come on with great rapidity, as if at the rate of from 40 to 80 miles an hour. They occur at all hours, oftentimes near sunset.

The sky is clear, and not a breath moving; presently a low bank of clouds is seen in the horizon, which you are surprised you did not observe before; a few seconds have passed, and the cloud has half filled the hemisphere: and now there is no time to lose—it is a dust-storm, and helter-skelter everyone rushes to get into the house in order to escape being caught in it.

The electric fluid continues to stream down the conducting wire unremittingly during the continuance of the storm, the sparks oftentimes upwards of an inch in length, and emitting a crackling sound; its intensity varying upon the force of the storm, and, as before said, more intense during the gusts.

Many dust-storms occur at Lahore and in the Punjaub, generally during the hot and dry months, as many as seven and nine in one month.

One that occurred last year in the month of August seemed to have come from the direction of Lica, on the Indus, to the west and by south of Lahore, and to have a north-easterly direction. An officer travelling, and at the distance of twenty miles or so from Lica, was suddenly caught in it; his tent was blown away, and he himself knocked down and nearly suffocated by the sand. He stated to me that he was informed by one resident at Lica, that so great was its force at the latter place, as to crack the walls of a substantial brick dwelling in which the above officer had lately resided, and to uproot some trees about.

The instant the insulated wire is involved in the electric current marked by the column of dust, down streams the electricity.

I have sometimes attempted to test the kind of electricity, and find that it is not invariably in the same state; sometimes appearing +, at other times —, and changing during the storm.

One day I caused the current to pass through a solution of cya-

nide of silver, so as to affect a small piece of copper, which was rapidly covered with a coating of silver, which upon drying peeled off. In this case the cyanide of silver was pure, without any salt; but in subsequent attempts to silver a wire in this way, I have not succeeded, only a very slight deposit taking place, which was not increased by long exposure to the influence.

But in all the cases I tried subsequent to the one first alluded to, the oxide of silver was dissolved in cyanide of potassium. In the course of time bright and minute crystals were formed, transparent and colourless, on a copper coin.

Yours truly,

P. BADDELEY,

Surgeon-Assistant, Lahore.

ON CERTAIN PHÆNOMENA OF FORCED DILATATION OF
LIQUIDS. BY M. MARCELLIN BERTHELOT.

If a somewhat strong capillary tube, closed at one end and drawn out at the other to a slender point, is filled with water at the temperature of 28° or 30° Cent.; if this tube is cooled down to 18° , so as to cause a certain quantity of air to enter it at the open point, and it is then closed, and again heated to 28° and gradually higher, after a certain time the air is completely dissolved. If cooled to 18° , the original temperature at which the tube contained at the same time gas and liquid, it is seen that the water continues to occupy the whole of the internal capacity, and maintains thus an invariable density between 28° to 18° . Its temperature may even be lowered still more. At this moment the least shock or collision, the least variation causes the instant reappearance, with a sort of ebullition, a slight noise, and a shock more or less perceptible, of the gas dissolved in the water. It dilates rapidly, and in less than a second has resumed its primitive volume at 18° . I have made the same observations with the following liquids, selected from all classes:—water, solutions of various salts and gases, solution of soda, various acids, alcohol, æther, acetone, Dutch liquid, essence of turpentine, oil of olives, creosote, sulphuret of carbon, chlorides of metalloids and metals, bromine. Mercury is the only liquid with which I have not succeeded, either in the presence of the air or *in vacuo*. A bubble of air remained several days in presence of the mercury without dissolving, at least completely, and that under pressures of 200 to 300 atmospheres, produced by preventing, for that length of time, the dilatation of the mercury due otherwise to an increased temperature of 8° or 10° .

In these phænomena there are two things very distinct. 1. An unstable supersaturation of the liquid by the gas, produced under the influence of the pressure. There are numerous examples of this order of facts. 2. A state of forced dilatation of the liquid: the latter, in fact, an instant before the vibration, fills the volume which the gas occupies an instant after conjointly with it, and this volume is the same which the dilated liquid filled on an elevation of temperature of 8 to 10 degrees and more. The variation of density thus produced is enormous; for water it is equal to $\frac{1}{4\frac{1}{2}0}$ of its volume at 18° ; for alcohol to $\frac{1}{9\frac{1}{3}}$, for æther to $\frac{1}{5\frac{1}{9}}$. Such an effect would be produced in an opposite direction only by a pressure of 50 atmospheres for

water, of 150 for æther. This phænomenon is very general, as is proved by the variety of the liquids on which I have operated. It probably accompanies all supersaturations, but at variable degrees and in various directions, without being capable of being proved.

Following the advice of M. Regnault, I endeavoured to separate the two facts, and to produce the dilatation of the liquids *in vacuo*. A peculiar apparatus enabled me to fill the tubes with liquids absolutely freed from air, and to close them without letting any gas enter. Under these new conditions, I reproduced the phænomenon of forced dilatation with water and æther, and have thus seen that it is independent of supersaturation. This permanence of the density of the liquids in an interval of temperature more or less considerable, appears to me attributable to the adhesion of the glass and the liquid: it is a force which is opposed to the division of this latter, and which can only be destroyed by an increase of the molecular attraction of the liquid for itself, an increase produced under the influence of cooling.—*Comptes Rendus*, June 24, 1850.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1850.

Chiswick.—June 1, 2. Very fine. 3, 4. Fine, but air excessively dry. 5. Slight haze: sultry. 6. Overcast: rain. 7. Cloudy and boisterous: showery. 8. Dull and cloudy: fine. 9—11. Very fine. 12. Fine: cloudy. 13. Cloudy: clear. 14. Uniformly overcast: rain: showery. 15. Rain: clear at night: frosty. 16. Clear: cloudy and fine. 17. Very fine. 18. Cloudless: very dry air: large distinct halo round the sun at noon. 19—22. Very fine. 23. Hot: quite cloudless. 24, 25. Hot, with slight dry haze. 26. Hazy: hot and sultry: heavy rain at night. 27. Rain: fine. 28. Hazy: rain. 29. Cloudy: very fine: clear and cold. 30. Fine: cloudy.

Mean temperature of the month	59°·26
Mean temperature of June 1849	59 ·30
Mean temperature of June for the last twenty-three years ...	60 ·88
Average amount of rain in June	1·88 inch.

Boston.—June 1. Cloudy. 2—5. Fine. 6, 7. Cloudy: rain P.M. 8, 9. Cloudy. 10, 11. Fine. 12, 13. Cloudy. 14. Cloudy: rain A.M. and P.M. 15, 16. Cloudy. 17. Cloudy: rain, with thunder and lightning A.M. 18, 19. Fine. 20. Cloudy. 21. Fine. 22. Cloudy. 23. Fine. 24. Fine: thermometer 88° 2 o'clock P.M. 25—27. Fine. 28. Fine: rain A.M. and P.M. 29, 30. Cloudy.

Applegarth Manse, Dumfries-shire.—June 1. Fine: fair: very warm. 2. Fine: very warm. 3. Fine: getting cloudy. 4. Fine: still cloudy. 5. Shower A.M.: thunder. 6. Shower A.M.: heavy rain P.M. and thunder. 7. Showery A.M.: fair P.M. 8. Showery all day. 9. Fair, but getting cloudy. 10. Slight shower early: fair P.M. 11. Slight shower early: fine day. 12. Rain and wind all day. 13. Rain during the night: fair all day. 14. Rain nearly all day. 15. Fair all day and fine. 16. Fair and fine: cloudy P.M. 17. Rain early: fine day. 18. Fine all day. 19. Cloudy, but fine. 20. Fair and fine: getting moist P.M. 21. Showery. 22. Cloudy: rain during night. 23—25. Very fine all day. 26. Very fine: fresh and invigorating. 27. Parching east wind. 28. The air highly electric. 29. The air highly electric: a few drops. 30. Rain P.M.: continued all night.

Mean temperature of the month	57°·6
Mean temperature of June 1849	53 ·3
Mean temperature of June for twenty-eight years	55 ·9
Rain in June for twenty years	3·16 inches.

Sandwich Manse, Orkney.—June 1. Fine. 2, 3. Fine: warm. 4. Fine. 5. Rain: fog. 6. Damp: cloudy. 7. Drops: showers. 8. Drops. 9. Drops: rain. 10. Fine: rain. 11. Showers: clear. 12. Rain: showers. 13. Drizzle: showers: drizzle. 14. Bright: drops. 15. Bright: clear. 16. Fine: clear: fine. 17. Fine. 18. Fine: cloudy. 19. Cloudy. 20. Showers: cloudy. 21. Rain: thunder: showers. 22. Bright: rain. 23. Cloudy. 24. Bright: clear. 25, 26. Cloudy. 27. Bright: cloudy. 28. Bright: clear. 29, 30. Bright: drops.

Days of Month.	Barometer.				Thermometer.				Wind.				Rain.			
	Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Dumfries-shire.	
	Max.	Min.	8½ a.m.	9 a.m.	9½ a.m.	8½ p.m.	Max.	Min.	8½ a.m.	Max.	9½ a.m.	8½ p.m.	Chiswick, 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1.	30.323	30.274	29.86	30.20	30.22	30.30	74	38	56	72	49½	53	ne.	ene.	sw.	s.
2.	30.366	30.355	29.88	30.29	30.30	30.33	76	40	60	73	49	59	e.	e.	s.	calm
3.	30.417	30.272	29.88	30.29	30.32	30.31	75	37	58.5	74	55½	66	e.	ese.	s.	s.
4.	30.214	30.078	29.73	30.13	30.00	30.13	78	44	61.5	72	52½	63½	e.	sse.	ssw.	n.
5.	30.005	29.814	29.49	29.84	29.67	29.90	78	55	59	65½	51	53	s.	sse.	ssw.	w.
6.	29.699	29.559	29.13	29.46	29.26	29.52	62	49	67	60	49	56½	sw.	sw.	e-s.	ene.
7.	29.737	29.539	29.00	29.17	29.50	29.27	66	43	60	59	50	55	sw.	swsw.	w.	w.
8.	30.000	29.886	29.43	29.70	29.87	29.54	69	41	60	59	45	54	sw.	w.	w.	w.
9.	30.197	30.141	29.68	29.96	29.95	29.85	74	40	59	60	49½	58	sw.	w.	sw.	s.
10.	30.118	29.890	29.60	29.91	29.79	29.84	80	50	68	62	52	60½	s.	w.	s.	calm
11.	29.899	29.844	29.34	29.65	29.74	29.50	80	46	65	62	53	55½	w.	w.	w.	w.
12.	29.918	29.696	29.33	29.50	29.30	29.36	68	47	63	59	51	52½	sw.	swsw.	ssw.	calm
13.	29.716	29.672	29.09	29.28	29.46	29.55	66	42	58.5	59	47½	48	w.	w.	w.	nnw.
14.	29.742	29.442	29.20	29.44	29.34	29.55	58	45	62	54½	48	45	sw.	sw.	sw-ne	nne.
15.	29.859	29.507	29.12	29.60	29.78	29.76	57	30	53	58	43½	47	ne.	n.	nne.	n.
16.	30.041	30.019	29.60	29.88	29.89	29.93	67	36	52	60	39½	49	ne.	e.	nne.	se.
17.	30.144	30.070	29.65	29.90	30.00	30.00	68	35	60	60	48	55	sw.	e.	s.	calm
18.	30.290	30.247	29.78	30.11	30.18	30.17	75	42	62	64½	50	53	w.	sw.	w.	sse.
19.	30.370	30.339	29.88	30.22	30.20	30.09	78	48	60	65	60½	59	n.	calm	sw.	swsw.
20.	30.278	30.187	29.76	30.13	30.07	29.99	81	47	69	59½	55	60	s.	calm	s.	sw.
21.	30.175	30.121	29.63	29.94	29.90	29.76	81	46	66.5	60	54	57	sw.	sw.	sw.	sw.
22.	30.206	30.177	29.60	29.92	29.99	29.76	82	47	69	59	54	56	w.	swsw.	s.	swsw.
23.	30.256	30.222	29.66	30.10	30.19	30.03	86	54	67.5	68	55½	54½	nw.	sw.	s.	sw.
24.	30.244	30.162	29.67	30.16	30.14	30.06	83	49	73	70	58½	54	e.	calm	s-sw.	sw.
25.	30.153	30.000	29.57	30.11	30.00	30.08	82	49	72	72	52	51	se.	ne.	sw.nw	w.
26.	29.954	29.851	29.44	29.91	29.93	29.95	84	53	72	67	50	53½	se.	swsw.	nw.	wnw.
27.	30.038	29.983	29.40	30.03	29.98	30.10	69	49	63	65	47	53	e.		e.	sse.
28.	29.819	29.631	29.35	29.82	29.63	29.71	69	53	65.5	65	48	63	e.	ne.	sw-w.	sw.
29.	29.858	29.738	29.22	29.57	29.62	29.48	66	43	58	61	44	54	sw.	nw.	w.	sw.
30.	29.947	29.963	29.40	29.65	29.60	29.46	71	55	60	57½	48	51	sw.	sw.	sw.	swsw.
														0.95		

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XVIII. *On the Composition of Beudantite.*

By JOHN PERCY, M.D., F.R.S.*

LEVY first described this mineral, and from his own examination of its physical characters, and Wollaston's determination of its composition, pronounced it to be a distinct species.

The following is Levy's description, the measurement of the crystals being omitted.

"This substance occurs in small crystals closely aggregated, of which the form is a slightly obtuse rhombohedron, with the summits truncated. Their colour is black at the surface, and their lustre somewhat resinous; but their fragments are translucent, and of a deep brown colour. The hardness is sensibly greater than that of fluuate of lime. When pounded, the colour is of a greenish-gray. The matrix seems to be the same substance in an amorphous state with veins of fibrous hæmatite; it comes from Hornhausen on the Rhine. I am indebted to Dr. Wollaston for the chemical examination of this mineral, the result of which is very interesting; the only substances he has been able to detect in it being oxide of lead and oxide of iron."—*Annals of Philos. N. S.* vol. xi. p. 195.

Recently, Descloizeaux has examined the crystallographic, and Damour the chemical properties of Beudantite (*Ann. de Ch. et de Phys.* t. x. p. 73, 3^{ième} S.). The crystals upon which they operated were taken from a specimen in the collection of the Ecole des Mines at Paris, and were of two kinds, green semi-transparent cubes, and bright black cubes, as seen by reflected light. Damour asserts that the former were composed of arsenic acid, oxide of iron and water, and

* Communicated by the Author.

were identical both in physical and chemical properties with cube-ore; and that the latter were principally composed of hydrated arseniate of sesquioxide of iron containing in accidental mixture oxide or sulphuret of lead.

Mr. Brooke having supplied me with some of Levy's original specimen of *Beudantite*, from which the portion examined by Wollaston was taken, I am now able to present quantitative analyses of this mineral. As, however, the quantity placed at my disposal was comparatively small, and as Wollaston mentioned only the presence of oxide of iron and lead, I shall consider it necessary to state somewhat minutely the results of my examination. It will be seen, by referring to Levy's description previously inserted, that only the black crystals of Descloizeaux agree with that description.

Qualitative Analysis.

1. Heated in the closed tube, colourless liquid was given off, which reddened litmus-paper, and produced a white precipitate in a solution of chloride of barium.

2. A yellow-brown bead was obtained by heating with borax in the outer flame of the blowpipe.

3. By heating on charcoal in the inner flame with carbonate of soda and a little borax, a dark-coloured bead was produced, from which, by trituration and l  vigation, numerous brittle metallic particles and a disc of soft metal like lead were obtained.

4. By roasting the metallic particles (3.) in an inclined open tube, the characteristic odour of arsenic was instantly perceived, and a very sensible quantity of white crystalline sublimate was formed.

5. The disc of soft metal (3.) dissolved without residue in dilute nitric acid. The solution, evaporated to dryness, gave a white crystalline residue, in the aqueous solution of which iodide of potassium produced a voluminous deep yellow precipitate.

6. The mineral completely dissolved in hydrochloric acid by the aid of heat, forming a brown solution like that of sesquichloride of iron.

7. Solution (6.) was not rendered turbid by dilution with water.

8. Solution (6.) gave a red-brown precipitate with ammonia, like sesquioxide of iron.

9. Solution (6.) gave a black precipitate with sulphuretted hydrogen in excess.

10. Solution (6.) gave a very sensible white precipitate with chloride of barium, not redissolved by dilution with water.

11. By Reinsch's test (electrotype copper being used), solution (6.) gave ample evidence of the presence of arsenic.

12. The mineral was fused with a mixture of carbonate of potash and soda. The product was washed with boiling water, when a clear colourless solution and a brown residue were obtained.

13. Reinsch's test applied to solution (12.) detected arsenic; by heat in an inclined open tube, the gray coating was removed, and a white, sparkling, crystalline sublimate obtained.

14. Marsh's test applied to solution (12.) also gave unequivocal evidence of the presence of arsenic in considerable quantity.

15. Solution (12.) was rendered acid by hydrochloric acid, and treated with sulphuretted hydrogen. The precipitate was digested with the strongest nitric acid; the solution was filtered, reduced by evaporation, and treated in Marsh's apparatus, as modified by the Prussian government (Chem. Gaz. vol. iii. p. 46), when an abundant deposit of metallic arsenic was obtained. This was converted into white sublimate, which was rendered yellow by sulphuretted hydrogen.

16. To the solution (15.) left, after separation of sulphuret and free sulphur, and reduced by evaporation, hydrochlorate of ammonia, and sulphate of magnesia were added. After some time numerous crystalline particles appeared on the surface of the liquid, and on the sides and bottom of the glass vessel. This crystalline precipitate, tested with molybdate of ammonia (Svanberg's process), gave a yellow precipitate.

17. The solution of the mineral in hydrochloric acid was treated with excess of ammonia and then digested with hydrosulphate of ammonia. The precipitate was treated with strong nitric acid. Sulphuretted hydrogen was passed through the diluted acid solution, which was afterwards filtered and boiled with nitric acid. Excess of ammonia was added. The precipitate was heated with carbonate of soda and nitrate of potash on platinum before the blowpipe, but no green coloration was produced.

18. The precipitate by ammonia (17.) was boiled with potash. The potash solution was rendered acid by hydrochloric acid, and afterwards alkaline by ammonia, but no sensible precipitate followed.

19. The brown residue (12.) was dissolved in hydrochloric acid. Sulphuretted hydrogen was passed through the solution, which was afterwards filtered and boiled with chlorate of potash. Ammonia was added. The filtrate was tested with oxalate of ammonia, but no turbidity followed.

20. The filtrate (19.) was tested with phosphate of soda,

but, after long standing, no trace of crystalline precipitate appeared.

First Analysis.

In this analysis the crystals employed were not selected under a lens, and were mixed with some amorphous brown matrix.

Weight of mineral after drying at 130°C ., 6.17 grains. The colour of the powder was light brown.

It was fused with more than 20 grains of a mixture of carbonate of potash and soda. The product was thoroughly washed with hot water and the solution filtered.

Solution (a) and Residue (b)

Solution (a). Colourless. Excess of hydrochloric acid was added. It was then boiled with a little sulphite of potash, after which sulphuretted hydrogen was passed through it. A reddish-brown precipitate was produced, which after standing all night in a warm place, was covered with a fine yellow precipitate.* Some lead had evidently been dissolved by the excess of alkaline carbonate*. The precipitate was washed with water and afterwards digested with hydrosulphate of ammonia.

A minute quantity of black matter remained undissolved upon the filter, upon which the sulphuret of lead subsequently separated from the insoluble residue (b) was collected.

The filtrate was treated with excess of hydrochloric acid, and left in a warm place more than twenty-four hours. The precipitate, which evidently consisted chiefly of free sulphur, was analysed in the usual way for arsenic. Weight of precipitate after drying until it ceased to lose weight, 2.08 grains. It was boiled with the strongest nitric acid. Globule of free sulphur, washed and dried, weighed 0.65. To the acid solution excess of chloride of barium was added. The sulphate of baryta obtained, washed, dried and ignited, weighed 7.55.

The solution, from which the arsenic had been precipitated as sulphuret, was reduced to a small volume by evaporation. Sulphate of magnesia, hydrochlorate of ammonia, and ammonia were then added. The crystalline precipitate was washed on the following morning with cold ammonia-water. Dried and ignited it weighed 0.14 gr.

Residue (b). It was dissolved in hydrochloric acid. Sulphuretted hydrogen was passed through the solution. The precipitate was collected on the filter before-mentioned. It was digested with the strongest nitric acid, a little sulphuric acid

* I have found that when a solution of carbonate of potash is long kept in a *flint-glass* bottle, it dissolves a very sensible amount of lead.

was added, and the whole evaporated to dryness and ignited. The product weighed 2.05 grs.

The solution from which the lead had been separated was boiled with chlorate of potash. Ammonia in excess was added. The precipitate, washed, dried and ignited, weighed 2.62 grs.

Second Analysis.

The particles of crystals were carefully selected under a lens. Weight 6.64 grains.

The mineral was digested in hot hydrochloric acid. Excess of ammonia was added to the solution, and afterwards excess of hydrosulphate of ammonia, when the whole was exposed to a gentle heat during many hours. The precipitate was washed on a filter with water containing a little hydrosulphate of ammonia, and afterwards treated with hot hydrochloric acid. Decomposition was complete. Through the solution, after filtration, sulphuretted hydrogen was passed for a considerable time. The precipitate was digested with the strongest nitric acid, and after the addition of a few drops of sulphuric acid, evaporated to dryness and ignited. The product weighed 2.66 grains.

The solution from which the lead had been separated by sulphuretted hydrogen was left in a warm place until the odour of that gas ceased to be detected. It was filtered, boiled with nitric acid, and treated with excess of ammonia. The precipitate, washed, dried and ignited, weighed 2.50 grs.

The hydrosulphate of ammonia solution was digested with excess of hydrochloric acid in a warm place for more than twenty-four hours. The precipitate, washed and dried at 100° C., until it ceased to lose weight, weighed 3.72 grs. The arsenic was determined indirectly in the usual way. The analytical data obtained are as follow:—

Sulphur and sulphuret analysed, weighed . . .	3.41
Ditto ditto, adherent to filter . . .	0.31
Globule of free sulphur	1.27
Sulphate of baryta	11.60

The solution from which the arsenic had been separated as sulphuret by hydrochloric acid, was reduced by evaporation, and treated with excess of chloride of barium. The precipitate, washed, dried and ignited, weighed 2.40 grs.

Of another portion of crystalline fragments selected under a lens, 5.34 grains were weighed.

The mineral was digested in hot hydrochloric acid. The solution was poured into a hot solution of chloride of ba-

rium. The precipitate, washed, dried and ignited, weighed 2.00 grs*.

Loss by calcination, determination of arsenic.—3.54 grs. of the mineral reduced to powder were heated in a small closed tube of Bohemian glass for a considerable time; first over the spirit-lamp, and afterwards at a bright red heat before the blowpipe. The loss in calcination was 0.40, the calcined mineral weighing 3.14 grs. This residue was red-brown, like sesquioxide of iron. It was dissolved in hot hydrochloric acid, and chloride of barium was added to the solution. The precipitate, washed, dried and ignited, weighed 0.90 gr.

The solution was freed from baryta by sulphuric acid: excess of ammonia was added, and afterwards hydrosulphate of ammonia. The determination of the arsenic was made as previously described. The analytical data are as follow:—

Sulphur and sulphuret analysed weighed	3.13
Sulphur and sulphuret adhering to filter	0.07
Free sulphur	0.50
Sulphate of baryta	17.24

Determination of Sulphuric Acid in part of the same specimen of mineral used in the last determination.

2.40 grs. were dissolved in hydrochloric acid. Chloride of barium was added. The precipitate, washed, dried and ignited, weighed 0.83 gr.

Results Tabulated.

First Analysis.

Weight of mineral 6.17 grs.

Arsenic by loss $0.39 = 6.32$ per cent. $= 9.68$ (As O⁵).

Phosphate of magnesia $0.14 = .09$ (PO⁵) $= 1.46$ per cent.

Sulphate of lead $2.05 = 1.51$ (PbO) $= 24.47$ per cent.

Sesquioxide of iron $2.62 = 42.46$ per cent.

Second Analysis.

Weight of mineral 6.64 grs.

Sulphate of lead $2.66 = 1.96$ (PbO) $= 29.52$ per cent.

* *On the Separation of Sulphuric Acid from Sulphate of Lead by Chloride of Barium.*—As the mineral under examination contained sulphuric acid and oxide of lead, the correctness of the preceding determination of sulphuric acid was tested by the following experiment. Of sulphate of lead, which had been prepared some years previously, and of the *absolute* purity of which I was not certain, 23.92 grains were weighed after heating, and dissolved in hot hydrochloric acid. The acid solution was poured into a hot solution containing more than 20 grains of chloride of barium. The sulphate of baryta, washed, dried and ignited, weighed 18.04. Now 23.92 of sulphate of lead contain of sulphuric acid 6.20, and 18.04 of sulphate of baryta contain of sulphuric acid 6.32. These results are sufficiently satisfactory.

Sesquioxide of iron $2.50 = 37.65$ per cent.

Arsenic by loss $0.59 = 8.88$ per cent. $= 13.60$ (As O⁵).

Sulphate of baryta $2.40 = 0.82$ (SO³) $= 12.35$ per cent.

Sulphuric Acid.

Weight of mineral 5.34 grs.

Sulphate of baryta $2.00 = 0.69$ (SO³) $= 12.92$ per cent.

Water, Arsenic and Sulphuric Acid.

Weight of mineral 3.54 grs.

Loss by calcination $0.40 = 11.30$ per cent.

Sulphate of baryta $0.90 = 0.31$ (SO³) $= 8.76$ per cent.

Arsenic by loss $0.26 = 7.34$ per cent. $= 11.24$ (As O⁵).

Determination of Sulphuric Acid in another portion of the same specimen as used in the last determination without calcination.

Weight of mineral 2.40 grs.

Sulphate of baryta $= 0.83 = 0.28$ (SO³) $= 11.67$ per cent.

Loss of sulphuric acid in calcination is $11.67 - 8.76 = 2.91$ per cent.

Water, therefore, is 11.30 (loss by calcination) $- 2.91$ (loss of sulphuric acid by calcination) $= 8.49$ per cent.

I.			II.		
		Oxygen.			Oxygen.
Oxide of lead . . .	24.47	1.75	29.52		2.12
Sesquioxide of iron . .	42.46	13.02	37.65		11.54
Sulphuric acid . . .	12.31*	7.37	12.35		7.39
Arsenic acid . . .	9.68	3.36	13.60†		4.72
Phosphoric acid . . .	1.46	0.82	not determined.		
Water	8.49	7.55	8.49		7.55
	98.87		101.61		

The preceding results prove that Beudantite is *chiefly* composed of sesquioxide of iron and oxide of lead, and so far confirm the accuracy of Wollaston's examination of it. They also confirm Damour's statement of the presence of arsenic and sulphur, and further demonstrate the presence of phosphoric acid. But the sulphur cannot, as Damour supposes, exist even in part as sulphuret, because the mineral dissolves without residue in hydrochloric acid; whereas if a sulphuret were present, there would be a residue of free sulphur, the result of the action of the sulphuretted hydrogen evolved by the decomposition of the sulphuret upon the sesquichloride of iron formed at the same time.

* Mean of three determinations.

† I think it probable that this determination of arsenic is very sensibly excessive. I have not given the mean of the several determinations, because I wished to put together the results obtained in the analysis of *one* portion of the mineral.

The quantitative results are less satisfactory than the qualitative. The difference between the proportions of oxide of iron and oxide of lead in the two analyses is considerable; but the following facts may serve in part to explain this discrepancy.

In the first analysis, the crystalline fragments were not selected under a lens, and were mixed with some of the amorphous brown matrix; while in the second, the pure crystalline fragments were alone selected under a lens as carefully as possible. Besides, the portions of mineral used in the two analyses were taken from different parts of Mr. Brooke's specimen, and were received by me at different times.

The differences also between the several determinations of arsenic are considerable; but, irrespective of the probable variation in composition of the several portions of mineral submitted to analysis, it is not to be expected that in analyses of such small quantities of matter as three or six grains, arsenic should be determined with precision. And the same may be said of the determinations of phosphoric acid. It is inferred that the arsenic exists as arsenic acid.

I wish it to be particularly understood, that I do not pretend to present these analytical results as more than approximate; for although I observed every precaution in the investigation, yet in the analyses of such a complex mineral as Beudantite, precision is not to be expected unless a larger quantity is operated on than I had at my disposal.

The oxide of lead appears to exist in combination with sulphuric acid, because in the mineral which has been strongly heated, sufficient sulphuric acid remains to saturate the oxide of lead. Thus 8.76 per cent. of sulphuric acid was found in the mineral after having been strongly heated, while before it contained 11.67 per cent. Now 8.76 of sulphuric acid contain 5.24 oxygen. The oxygen of the oxide of lead in the first analysis is 1.75, or precisely one-third of that of the acid; that is, in the ratio which exists between the oxygen of the base and that of the acid in neutral sulphate of lead. In the second analysis it is less than one-third; still the difference is within the limit which, in the present case, may fairly be assigned to error of analysis.

Again, the sulphuric acid could only be combined in small proportion with oxide of iron; for otherwise a much larger quantity of acid must have been evolved at the high temperature to which the mineral was subjected in the experiment to determine the loss by calcination previously recorded.

It may therefore be inferred, that the oxide of lead in Beudantite is combined with sulphuric acid.

If this inference be admitted, it follows that the excess of sulphuric acid beyond that required to saturate the oxide of lead, is, together with the arsenic and phosphoric acids, combined with sesquioxide of iron, of which there is much more than sufficient to neutralize those acids. The mineral may therefore be considered as a mixture of sulphate of lead, and of arseniate and sulphate of sesquioxide of iron with excess of base.

The sulphate of lead cannot be regarded as in combination with the salts of iron, because no analogous combination is known. Nor is it more probable that arseniate and sulphate of sesquioxide of iron should exist in combination with each other. Hence the mineral would appear to be a simple mixture of three distinct salts. But it is crystallized; and the crystalline form certainly is very similar to, if not identical with, that of cube-ore. Levy maintained that it was an obtuse rhombohedron with the vertical angle truncated. Descloizeaux, on the other hand, asserts that the crystals are cubes similar in all respects to those of cube-ore from Cornwall. (*Ann. de Chim. et de Phys.*, vol. x. p. 77.)

Professor Miller, however, has not been able to obtain satisfactory measurements of the crystals. Now, as Beudantite contains in very sensible proportion the elements of cube-ore,—as the form of the crystals, to say the least, very nearly resembles that of cube-ore,—and as neither sulphate of lead nor sulphate of sesquioxide of iron forms crystals at all similar to those of cube-ore, the most probable conclusion seems to be that of Damour, namely, that *Beudantite is nothing more than cube-ore containing an accidental mixture of much foreign matter*. Such mixtures, as Damour observes, are frequently met with in the mineral kingdom, and present a very interesting subject of inquiry. An extended investigation to determine the amount of foreign matter which may enter into the composition of crystals, and its power of distorting or modifying the crystalline form, would certainly yield many valuable results.

XIX. On the Triads made with Fifteen Things.

By the Rev. THOMAS P. KIRKMAN, M.A.*

THE neatest method of writing the solution of the problem mentioned at page 52 of this volume is the following:—

$a_1a_2a_3$	$a_1b_1c_1$	$a_1d_1e_1$	$a_1b_2d_2$	$a_1c_2e_2$	$a_1b_3e_3$	$a_1c_3d_3$
$b_1b_2b_3$	$a_2b_2c_2$	$a_2d_2e_2$	$a_2b_3d_3$	$a_2c_3e_3$	$a_2b_1e_1$	$a_2c_1d_1$
$c_1c_2c_3$	$a_3d_3c_3$	$a_3b_3c_3$	$a_3c_1e_1$	$a_3b_1d_1$	$a_3c_2d_2$	$a_3b_2e_2$
$d_1d_2d_3$	$b_3d_1c_2$	$d_3b_1c_2$	$b_1c_3e_2$	$c_1b_3d_2$	$b_2c_3d_1$	$c_2b_3e_1$
$e_1e_2e_3$	$c_3d_2c_1$	$e_3b_2c_1$	$d_1c_2e_3$	$e_1b_2d_3$	$e_2c_1d_3$	$d_2b_1e_3$

* Communicated by the Author.

The last three pairs of columns exhibit three systems of subindices, which *systems* may be cyclically permuted, the letters remaining undisturbed: this gives us two sets more each of six final columns, making three sets of six columns, any set of which being added to the primary column will solve the problem. If we now permute the letters *abcde* cyclically in these 6×3 columns, we shall obtain four times 6×3 columns more, making 6×14 columns in all, additional to the system of seven above written; so that we can complete the primary column into a solution in fifteen ways, and this without repeating any triad: by this process we shall exhaust the 455 triads that are possible with fifteen things. All this I have long known; but it never occurred to me to observe that the additional 6×14 were exactly 12×7 , and thus to make the pleasing variation, by which Mr. Sylvester has extended the puzzle over the remaining twelve weeks of the quarter. We see that this is, as Mr. Cayley expected, a matter of cyclical permutation; not of thirteen (a negative which he has proved), but of five letters.

I obtained this property of the triads made with fifteen things four years ago, by observing that, if you substitute in Q_{15} , at page 195 of the second volume, N. S. of the Cambridge and Dublin Mathematical Journal, in the place of the small letters, the second instead of the first arrangement of D_8 given on the preceding page, Q_{15} can be broken up into seven columns of five triads each, so as to solve this problem of the fifteen young ladies; but Q_{15} , as it stands at page 195, cannot be so broken up. The solution of Mr. Cayley at page 52 above is obtained by a like tentative process; and, in fact, all the solutions are of the same form, and can be made identical with the one above written, by disturbing the alphabetical order, or that of the subindices in certain triads, or both, in the first column. The question has yet to be mathematically treated: I do not feel satisfied with knowing how to form thirty-five triads, which are found on *trial*, but not certainly proved before trial, to be capable of the required arrangement. We want to know, before trial, *why* a school of fifteen *can* thus be marched out till every pair have walked together, and why a school of twenty-one *can* also, or *cannot*. I have a strong opinion, but will not undertake to prove the negative, that twenty-one cannot be thus arranged.

Permit me, in conclusion, to enunciate the following propositions:—

Theor. I. $5 \times 3^{m+1}$ symbols can be arranged in $\frac{1}{2}(5 \cdot 3^{m+1} - 1)$ columns of triads, each column containing all the symbols, and so that every duad shall be once, and once only, employed.

Theor. II. If r be any prime number, and otherwise not,

r^{m+1} symbols can be arranged in $(r^{m+1}-1):(r-1)$ columns of r -plets, each column containing all the symbols, and so that every duad shall be once, and once only, employed.

Theor. III. If r be any prime number, (r^2+r+1) symbols can be formed into (r^2+r+1) $r+1$ -plets, so that every duad shall be once, and once only, employed.

Theor. IV. If r be any prime number, and r^2+r+1 be prime, $(n=) (r^2+r+1)(r+1)$ symbols can be formed into $n.n-1:r.r+1$ $r+1$ -plets, so that every duad shall be once, and only once, employed.

Theor. V. Nine symbols can be arranged in seven sets each of four columns of triads, every column containing the nine symbols once, and so that no triad shall be twice employed.

Theor. VI. If $R=A^aB^bC^c \dots L^lM^mN$ be any integer, of which $A, B, C, \dots L, M$ are prime factors in decreasing order, N being any number less than M , R symbols can be arranged in $(A^a-1):(A-1)$ columns of A -plets, $+ A^a.(B^b-1):(B-1)$ columns of B -plets, $+ A^aB^b.(C^c-1):(C-1)$ columns of C -plets, $+ \dots + A^aB^bC^c \dots L^l(M^m-1):(M-1)$ columns of M -plets, $+ A^aB^bC^c \dots L^lM^m$ columns of N -plets, and this so that each column shall exhibit all the R symbols, and that every duad shall be once employed and repeated nowhere.

Croft Rectory, near Warrington,
August 6, 1850.

XX. *On the Action of the Soap-Test upon Water containing a Salt of Magnesia only, and likewise upon Water containing a Salt of Magnesia and a Salt of Lime.* By DUGALD CAMPBELL, F.C.S. London*.

FOR some time back I have been engaged in analysing specimens of water—determining by actual experiment, besides the salts of the alkalies which they contain, the amount of phosphate of iron and salts of lime and magnesia in a gallon, testing them at the same time with reference to their *degrees of hardness*, according to the plan adopted by Prof. Clark in the specification of a patent printed in the Repertory of Patent Inventions for 1841.

When the quantities of phosphate of iron and salts of lime and magnesia found in a gallon of water were calculated each into its equivalent of chalk and added together, I had anticipated that their amount would correspond, or nearly so, with

* Communicated by the Author, having been read before the British Association, August 1850.

the degrees of hardness of the water as ascertained by the soap-test, this being the rule given by Professor Clark in a private circular to his chemical friends to "infer the degrees of hardness from an ordinary analysis." But in this I was mistaken. I therefore thought it advisable to make some experiments with the soap-test upon water of which I knew the actual composition.

The soap-test I employed in the following experiments was prepared with the utmost care, the alcohol having always been digested with carbonate of potash for many hours before being reduced to proof-spirit. The soap corresponded with what Prof. Clark advises, nearly 1 oz. in a gallon of proof-spirit, giving a soap-test, thirty-two test measures of which formed a perfect lather with 100 like measures of a standard solution of 16° hardness of lime, which remained all over the water for five minutes when first placed after shaking, and which could be renewed at any time by again agitating it. The soap-test was likewise proved by standard solutions of lime of 12°, 8°, 6°, 4°, 2° hardness.

The distilled water employed in making standard solutions was selected with much care, and tested in various ways to be sure of its purity.

The first experiments which I made were upon water containing a salt of magnesia in different proportions.

To prepare such solutions, 19·2 grains perfectly pure and dry sulphate of magnesia (MgO, SO_3), which is the equivalent quantity of that salt to 16 grains carbonate of lime, were dissolved in a gallon of distilled water. This was considered as a standard of magnesia equal to 16° hardness of lime, or a standard of magnesia equal to 16°. Fifteen other standard solutions were prepared from this, as follows: by taking *one* measure of the 16° standard, and adding fifteen like measures of distilled water to it, we have a standard of 1° magnesia; with *two* measures of original standard and fourteen like measures of distilled water added to it, we have a standard of 2° magnesia; and so on up to 15°.

The results which I obtained (see Table No. 1, p. 173) when treating these solutions with soap-test, were such as to induce me to proceed with experiments upon mixed standard solutions of magnesia and lime. These I prepared as follows:—Magnesia standards ranging from one to sixteen were made each to contain double the amount of the magnesian salt that the previous standards contained. Lime standards were then made of 32°, 24°, 16°, 12°, 8°, 4° hardness in a gallon. When fifty measures of these lime standards were mixed with fifty like measures of the last magnesian standards, I then consi-

dered that I had standard solutions of 16°, 12°, 8°, 6°, 4°, 2° hardness of lime, plus 1°, 2°, 3° magnesia, and so on up to 16°.

The Tables Nos. 2 to 7 give the number of soap-test measures which I have found requisite to produce a perfect and renewable lather, when shaken with one hundred test measures of these solutions in a phial capable of containing about five fluid ounces.

Table No. 1.

Table No. 2.

Magnesian standard.	Soap-test measures. Magnesia.	Soap-test measures. Lime.	Lime standard.	Magnesian standard.	Soap-test measures.
1	3.2	3.2	16°	1°	31.6
2	5.4	5.4	16	2	31.6
3	7.6	7.6	16	3	31.5
4	9.5	9.6	16	4	31.5
5	11.5	11.6	16	5	31.4
6	13.4	13.6	16	6	31.3
7	14.8	15.6	16	7	31.2
8	15.4	17.9	16	8	31.0
9	16.0	19.4	16	9	30.8
10	16.9	21.3	16	10	30.5
11	17.7	23.1	16	11	30.2
12	18.3	24.9	16	12	29.8
13	18.7	26.7	16	13	29.4
14	19.1	28.5	16	14	28.9
15	19.4	30.3	16	15	28.4
16	19.6	32.0	16	16	27.9

Table No. 3.

Table No. 4.

Lime standard.	Magnesian standard.	Soap-test measures.	Lime standard.	Magnesian standard.	Soap-test measures.
12°	1°	24.6	8°	1°	17.3
12	2	24.5	8	2	17.3
12	3	24.4	8	3	17.3
12	4	24.3	8	4	17.3
12	5	24.1	8	5	17.3
12	6	23.9	8	6	17.2
12	7	23.4	8	7	17.1
12	8	22.8	8	8	16.9
12	9	22.5	8	9	16.0
12	10	22.3	8	10	15.0
12	11	22.1	8	11	14.6
12	12	22.0	8	12	14.5
12	13	21.9	8	13	14.4
12	14	21.8	8	14	14.3
12	15	21.7	8	15	14.2
12	16	21.7	8	16	14.1

Table No. 5.

Lime standard.	Magnesian standard.	Soap-test measures.	Additional soap-test measures.
6	1	13.4	
6	2	14.4	
6	3	14.8	
6	4	15.1	
6	5	15.3	
6	6	15.4	
6	7	14.0	+ 2.4
6	8	12.1	+ 12.1
6	9	11.9	+ 12.7
6	10	11.7	+ 13.5
6	11	11.4	+ 14.3
6	12	11.2	+ 15.0
6	13	11.0	+ 15.7
6	14	10.9	+ 16.1
6	15	10.8	+ 16.4
6	16	10.7	+ 16.7

Table No. 6.

Lime standard.	Magnesian standard.	Soap-test measures.	Additional soap-test measures.
4	1	10.7	
4	2	12.8	
4	3	14.6	
4	4	16.3	
4	5	15.9	+ 1.9
4	6	15.4	+ 4.0
4	7	14.3	+ 6.8
4	8	11.9	+ 11.0
4	9	9.7	+ 15.2
4	10	8.2	+ 18.7
4	11	7.8	+ 21.0
4	12	7.6	+ 22.2
4	13	7.4	+ 23.3
4	14	7.2	+ 23.9
4	15	7.1	+ 24.4
4	16	7.0	+ 25.0

Table No. 7.

Lime standard.	Magnesian standard.	Soap-test measures.	Additional soap-test measures.
2	1	7.5	
2	2	9.5	
2	3	11.3	
2	4	13.2	
2	5	14.4	
2	6	16.0	
2	7	17.8	
2	8	17.0	+ 1.6
2	9	16.3	+ 3.2
2	10	15.3	+ 5.1
2	11	12.5	+ 9.9
2	12	12.2	+ 10.6
2	13	11.9	+ 12.1
2	14	11.6	+ 13.6
2	15	11.2	+ 15.2
2	16	10.8	+ 16.9

Remarks upon the Tables.

Table No. 1. *Magnesia Standards*.—On comparing the number of soap-test measures which I have found requisite to produce a perfect lather with the magnesia standards, with the number of soap-test measures which will cause a similar effect with the corresponding standards of lime*, it will be

* These are the same which are given by Professor Clark in a private circular to his chemical friends, and which I have introduced into Table No. 1, to serve for comparison with the results I have obtained, and which are detailed in that and the other tables.

seen that only as far as the 6th degree do the magnesia correspond, or nearly so, with the lime standards. And as the strength of the solutions increase, so much more do the differences increase, until a standard of 16° magnesia, and a standard of 9° lime, require nearly the same number of soap-test measures to produce a proper lather in each.

Some experience is requisite in making experiments upon water containing magnesian salts alone, and likewise upon magnesian water mixed with lime salts. Magnesian salts in water more or less produce a curd with soap which interferes in some measure with the lather. They likewise do not act so readily upon soap as the salts of lime do; so that in making an experiment, a good deal more agitation is required to produce a lather. I have found it best to add the soap-test little by little with agitation; in fact, all solutions should be treated in the same manner as when a water of unknown hardness has to be tested. If the requisite quantity of soap-test be added at once to a standard, a good deal of shaking is required to produce even a curd: this is more the case as the quantity of magnesia increases, but by additional shaking the lather appears. A proper lather once having been obtained, it can with very moderate shaking be restored again, not only for hours but for days afterwards, and improved as to stability. The addition however of a little more soap-test with gentle agitation, instead of at once improving the lather, has the contrary effect, and a curd appears instead of the lather, or apparently the water is rendered hard, and it is only by very considerable agitation that the lather can be restored; when restored, it is a better lather than before the addition of the last portion of soap-test: this operation may be repeated upon the same solution several times and with similar results, by proceeding as above described, until a considerable amount of soap-test measures have been added above the quantity requisite to produce a proper lather. This is more particularly the case in the higher degrees.

Table No. 2. 16° *Lime plus Magnesia degrees*.—On referring to this table, it will be seen that the solution of 16° lime plus 1° magnesia, actually does not require so much soap-test to form a perfect lather as does the solution of 16° of lime alone, and as the magnesia increases in the solutions, the soap-test measures requisite to produce a lather diminish, until the solution of 16° lime plus 16° magnesia gives a lather with 27.9 measures, *i. e.* 4.1 soap-test measures less than is required by the standard of lime of 16° hardness. The action of a few more soap-test measures, when added to solutions in which a perfect lather had once been established, of appa-

rently rendering them hard, was noticed in these as in the former solutions.

Table No. 3. 12° *Lime plus Magnesia degrees*.—It will be observed in this table that the magnesia acts similarly, although scarcely so powerful as in the last experiments; but on the whole, the action of magnesia throughout this series of experiments very much accords with its action in the former.

Table No. 4. 8° *Lime plus Magnesia degrees*.—On referring to this table, it will be seen that the magnesia acts upon the lime standard much as in the last experiments.

Table No. 5. 6° *Lime plus Magnesia degrees*.—On examining this table, we find now that the magnesia begins to act differently towards the lime than it has hitherto done; the 6° lime plus 1° magnesia is now equal in soap-test measures to 6° lime, at least the difference ($0\cdot2$) may be considered within the limits of error of experiment; from 2° mag. up to 6° there is a gradual rise, but at 7° it begins to fall, and continues to do so throughout the remaining solutions. I may remark that the lathers from 7° to 16° are not produced with the soap-test given in table, except with considerable agitation, and although thin, are perfectly free from curd and are reproducible at any future time by agitation. When a perfect lather has been produced by the minimum quantity of soap-test, the addition of a small quantity more will reduce the lather to a curd; great agitation may produce the lather again, and stronger than it was before the last addition of soap-test: if beyond a certain amount of soap-test has been added, a new action shows itself, which I have failed to notice in any of the previous solutions, which is, that shaking cannot restore the lather properly again until a certain amount of soap-test has been added; the additional quantities of soap-test I have had to add are in the table, and opposite the solutions in which I noticed this peculiarity; how to account for it I know not, and have not as yet had time to investigate into it. I am unable to account for the wide difference in the *amount of additional soap-tests* between the numbers 7° and 8° ; as the other numbers seem to follow regularly, it may be owing to another point in some of these solutions at which much agitation and less soap-test would produce a lather, but which, although I have made many experiments, I have not succeeded in hitting upon. When to the lather established after the additional soap-tests a small quantity of soap-test is added, it renders it curdy, but shaking will re-establish the lather again.

Table No. 6. 4° *Lime with Magnesia degrees*.—On looking over this table, it will be noticed that the magnesia comes into

action more fully, and for the first time the standard of lime with 1° magnesia requires more soap-test to form a lather than does the standard of lime alone; still it does not amount to what would be necessary for 5° lime. The soap-test measures required to produce perfect lathers with 4°, 5°, 6° and 7°, are greater than was required in the last series of experiments upon these numbers with the 6° standard of lime. The same peculiarities of the soap-test upon some of these solutions were observed in this set of experiments as in the last.

Table No. 7. 2° *Lime with Magnesia degrees*.—On examining this table, it will be seen that the solutions of this standard of lime with the first four magnesia degrees appear to require nearly as many soap-test measures as if they were entirely lime standards. It will likewise be observed, on comparing Table 4 with this table, that 7°, 8°, 9° magnesia with 8° lime, do not take each so many soap-test measures as 7°, 8°, 9° magnesia with 2° lime, to form proper lathers. Again, in Table No. 5, it will be seen that each of the magnesia degrees from 6° to 16°, with the standard of lime which is 6°, do not take so many soap-test measures to form proper lathers, as do these magnesian solutions with 2° hardness of lime. On referring to Table 6, the same remark applies to these solutions as to the last, that is, that 2° lime with each of the standards of magnesia from 6° to 16° require more soap-test to produce proper lathers than solutions of 4° lime with each of these magnesian standards require. I would call attention again to Table 7, to notice a rapid descent in soap-test measures between 10° and 11° magnesia; I have made many experiments if possible to explain this, but without success. I can only observe, that in certain stages of a series of experiments of a standard of lime with magnesia, it requires much care, attention and observation, to mark when the lather prevails over the curd, and to obtain at this particular stage uniform lathers, more especially when the soap inclines to form a curd in a solution, together with a lather, at the same time. If these points are not attended to, and no perseverance employed in agitating the solutions, uniform results cannot be expected. The other observations I have made on the preceding series of waters are applicable to these. The inferences which I draw from the foregoing experiments are as follows:—

1st. That water containing sulphate of magnesia alone acts towards the soap-test in producing with it a perfect lather, similarly or nearly so, as does water containing a lime salt alone, but only when the equivalent of magnesia salt does not exceed 6 grs. of carbonate of lime in a gallon of water.

2nd. That the degrees of hardness of an ordinary water

cannot be inferred by the rule :—Compute the grains of lime, magnesia, oxides of iron, alumina, in a gallon of water, each into its equivalent of chalk : the sum of these equivalents will be the hardness of the water.

3rd. That the degrees of hardness of a water containing magnesia and lime salts, as shown by the soap-test as it is now applied, cannot in almost every case be taken as representing the amount of these salts in the water ; nor in nearly every instance can it be considered as giving the amount of lime in a water when magnesia is present.

4th. That water might show by the soap-test a small degree of hardness in comparison to the considerable quantities of salts of magnesia and of lime it might contain, and trusting to this method of analysis alone when selecting water for ordinary use and for steam purposes, might lead to a water being selected which might not be conducive to the general health, and which would leave considerable deposit in vessels in which it was boiled,—a great deterioration to its use in steam generating.

XXI. *On an Accelerating Process in Photography.*

*By J. MIDDLETON, F.G.S.**

THE following method of preparing sensitive paper, may, perhaps, be welcome to photographers on account of the great sensibility which it confers ; it has the additional recommendation, moreover, of being very simple and constant in its results.

I beat up albumen of the egg of the duck till it becomes liquid, and then mix it with water in the proportion of eighty grains of the former to an ounce of the latter. I add to this solution iodide of potassium in the proportion of twenty-five grains to the ounce. Prior to the application of this solution I wash the size from the side to be rendered sensitive, by means of a camel-hair brush, and when dry I float the paper on the solution, for from three to four minutes, and when drained and dried I lay it aside for use.

When about to be used for taking a picture, the paper, prepared as above directed, is to be washed with aceto-nitrate of silver, in the proportion of sixty grains of nitrate and eighty grains of acetic acid to an ounce of water (Talbot's strength). I apply the solution with a glass rod, in the manner recommended some time since by a writer in the *Philosophical Magazine*, using about forty grains of it to a

* Communicated by the Author.

quarto page, and allow the paper to dry in the dark; it is now ready for the camera. While applying the sensitive coat, as also while bringing out the picture, I take the precaution to use a yellow light. I find that from ten to fifteen seconds is, with ordinary sun-light, sufficient exposure, the latter being generally too great.

When the picture has been taken no trace of it appears on the paper, but it comes speedily out on the application of a saturated solution of gallic acid. I turn up the edges of my paper and pour the solution on till the paper is entirely covered, and keep it so till the picture has come sufficiently out, when I fix it in the usual way.

I find that if bromide of potassium be substituted for iodide of potassium, in the first process, a picture is obtained; but the time of exposure required is then about a minute. Again, bromide or chloride of potassium does not serve to accelerate, as in the ordinary processes, but the contrary; gallic acid too, added to the aceto-nitrate, destroys sensitiveness. I find also, that if the albumen be dried, and afterwards dissolved up and used as above described, it has lost its photographic value; a circumstance, which would seem to indicate that photographic properties are connected with or dependent upon molecular arrangement.

The employment of albumen in photography is not, I believe, new: it has not, however, so far as I am aware, been used in the way or with the effect stated above.

Agra, 18th June, 1850.

XXII. *Algerite*, a new Mineral Species.

By RICHARD CROSSLEY, Esq.*

A DETAILED description of this mineral by Mr. F. Alger, accompanied with an analysis by Mr. T. S. Hunt, was published in the American Journal of Science for July 1849, vol. viii. I have since, at the request of Mr. Alger, made a re-examination. The results obtained vary but little from those of Mr. Hunt, though this variation is essential to the formula. *Algerite*, named by Mr. Hunt in honour of its discoverer, Mr. Alger, is found in the town of Franklin, Sussex County, New Jersey. It is sparingly disseminated in prismatic crystals through a bed of pure crystalline limestone. The crystals when undecomposed are of a honey-yellow colour, and more or less penetrated by the matrix: some are contaminated by graphite, and others are encrusted with idocrase,

* Communicated by W. G. Lettsom, Esq.

which mineral also occurs at this locality. The honey-yellow crystals give a spec. grav. 2·78. Hardness from 3 to 3·5. Brittle, subtranslucent. Before the blowpipe alone it fuses readily to a white blebby glass. With soda it fuses to a dirty white slag. With borax and phosphorus salt it gives a bead, faintly tinged by iron, enclosing a siliceous skeleton. Heated in a closed tube it gives off water which reacts feebly alkaline, and the powder changes in colour from light buff to light brown. It is perfectly attackable by a mixture of sulphuric and hydrochloric acids.

Crystals of the mineral, carefully cleaned, gave for its composition—

Silica	49·96
Alumina	24·41
Peroxide of iron	1·48
Magnesia	5·18
Carbonate of lime . . .	4·21
Potash	9·97
Water	5·06
	100·27

The amount of carbonic acid directly determined is equal to that in the carbonate of lime obtained. It is therefore evident that the lime is not a constituent of the mineral.

Deducting the carbonate of lime, and calculating the remaining members for per-centage proportions, we shall have for the composition of *Algerite*,—

		Proportion of oxygen.	Ratio.
Silica	52·00	27·00	7
Alumina	25·42	11·88	3
Peroxide of iron . . .	1·54	47	
Magnesia	5·39	2·08	1
Potash	10·38	1·75	
Water	5·27	4·68	1
	100·00		

The above composition is very well represented by the formula



and there being no previously known mineral which gives the above characters, composition and formula, *Algerite* must necessarily rank as a new species.

Boston, Mass. U.S., April 29, 1850.

XXIII. *On the Diffusion of Liquids.*

By THOMAS GRAHAM, F.R.S., F.C.S.*

ANY saline or other soluble substance, once liquefied and in a state of solution, is evidently spread or diffused uniformly through the mass of the solvent by a spontaneous process.

It has often been asked whether this process is of the nature of the diffusion of gases, but no satisfactory answer to the question appears to be obtained, owing, I believe, to the subject having been studied chiefly in the operations of endosmose, where the action of diffusion is complicated and obscured by the imbibing power of the membrane, which is peculiar for each soluble substance, but no way connected with the diffusibility of the substance in water. Hence also it was not the diffusion of the salt, but rather the diffusion of the solution, which was generally regarded. A diffusibility like that of gases, if it exists in liquids, should afford means for the separation and decomposition even of unequally diffusible substances, and being of a purely physical character, the necessary consequence and index of *density*, should present a scale of densities for substances in the state of solution, analogous to vapour densities, which would be new to molecular theory.

M. Gay-Lussac proceeds upon the assumed analogy of liquid to gaseous diffusion in the remarkable explanation which he suggests of the cold produced on diluting certain saline solutions, namely, that the molecules of the salt expand into the water like a compressed gas admitted into additional space.

The phænomena of solubility are at the same time considered by that acute philosopher as radically different from those of chemical affinity, and as the result of an attraction which is of a physical or mechanical kind. The characters indeed of these two attractions are strongly contrasted. Chemical combination is uniformly attended with the evolution of heat, while solution is marked with equal constancy by the production of cold. The substances which combine chemically are the dissimilar, while the soluble substance and its solvent are the like or analogous in composition and properties.

In the consideration of solubility, attention is generally engrossed entirely by the quantity of salt dissolved. But it is necessary to apprehend clearly another character of solution, namely, the degree of force with which the salt is held in solution, or the intensity of the solvent attraction, quite irre-

* From the Philosophical Transactions for 1850, part i.; having been received by the Royal Society November 16, and read December 20, 1849.

spective of quantity dissolved. In the two solid crystalline hydrates, pyrophosphate of soda and sulphate of soda, we see the same ten equivalents of water associated with both salts, but obviously united with unequal degrees of force, the one hydrate being persistent in dry air and the other highly efflorescent. So also in the solutions of two salts which are equally soluble in point of quantity, the intensity of the attraction between the salt and the water may be very different, as exemplified in the large but feeble solubility in water of such bodies as the iodide of starch or the sulphindylate of potash, compared with the solubility of hydrochloric acid or of the acetate of potash, which last two substances are capable of precipitating the two former, by displacing them in solution. Witness also the unequal action of animal charcoal in withdrawing different salts from solution, although the salts are equally soluble; and the unequal effect upon the boiling-point of water produced by dissolving in it the same weight of various salts. Besides being said to be small or great, the solubility of a substance has also therefore to be described as weak or strong.

The gradations of intensity observed in the solvent force are particularly referred to, because the inquiry may arise how far these gradations are dependent upon unequal diffusibility; whether indeed rapidity of diffusion is not a measure of the force in question.

I have only further to premise, that two views may be taken of the physical agency by which gaseous diffusion itself is effected, which are equally tenable, being both entirely sufficient to explain the phænomena.

On one theory, that of Dr. Dalton, the diffusibility of a gas is referred immediately to its elasticity. The same spring or self-repulsion of its particles which sends a gas into a vacuum, is supposed to propel it through and among the particles of a different gas.

The existence of an attraction of the particles of one gas for the particles of all other gases is assumed in the other theory. This attraction does not occasion any diminution of volume of gases on mixing, because it is an attraction residing on the surfaces of the gaseous molecules. It is of the same intensity for all gases, hence its effect in bringing about intermixture is dependent upon the weight of the molecules of the gases to be moved by it; and the velocity of diffusion of a gas comes to have the same relation to its density on this hypothesis as upon the other*.

* Both of the molecular theories of the diffusion of gases were first publicly explained, and at the same time ably discussed, with the reference to

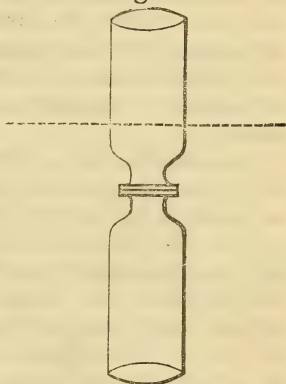
The surface attraction of molecules assumed will recall the surface attraction of liquids, which is found necessary to account for the elevation of liquids in tubes and other phenomena of capillary attraction.

(1.) An early preliminary experiment was made upon the liquid diffusion of a body, with whose diffusion as a gas we are already well acquainted, namely, carbonic acid dissolved in water.

Two half-pound stoppered glass bottles were selected, of which the mouths were 1·2 inch in diameter, and the lips were ground flat so as to close tight when applied together (fig. 1).

One of them, placed firmly in an upright position, was filled to the base of the neck with carbonic acid water. Over this distilled water was poured, care being taken to disturb the liquid below as little as possible, in filling up the neck. The second bottle, filled with distilled water and inverted upon a glass plate, was slipped over the first at the water-trough. The solution of carbonic acid in the lower bottle was thus placed in free communication by an aperture of 1·2 inch, with an equal volume of pure water in the upper bottle. It was expected that the carbonic acid would be found, in time, equally diffused through both bottles.

Fig. 1.



After forty-eight hours, the upper inverted bottle was again slipped off from the lower one, upon a glass plate, and the ratio of the gas found in the upper to that in the lower bottle determined by the weight of carbonate of baryta which the liquids of the two bottles afforded respectively. It was as 1·18 to 12·80 (about 1 to 11), instead of the ratio of equality, which would undoubtedly be the ultimate result of diffusion, were sufficient time allowed.

After five days, in a second experiment with a weaker solution of carbonic acid, the gas was found to be distributed—

In upper bottle . . .	1·63
In lower bottle . . .	8·44

or in the proportion of 1 to 5 nearly.

the law of diffusion which had been drawn from observation, by my late friend Mr. T. S. Thomson of Clitheroe. A decided preference was given by Mr. Thomson, and also by the late Mr. Ivory, to the last, or the attraction theory of diffusion, over that of gases being *vacua* to each other. See Phil. Mag., 3rd series, vol. xxv. pp. 51, 282.

In other experiments where the liquid in the upper bottle was a solution in water of nitrous oxide gas, instead of pure water, the carbonic acid of the lower bottle was also observed to diffuse into the liquid above it, as freely as it did into pure water in a comparative experiment; the ultimate ratios being 1 to 0·12 in the nitrous oxide liquid, and 1 to 0·10 in the water experiment.

With the necks of the pair of bottles occupied by sponge charged with distilled water, the diffusion of the carbonic acid of the lower bottle proceeded with little change in its rapidity, or in the result when nitrous oxide was placed above it. The carbonic acid found in the upper bottle, and which had diffused into it from the lower, was 0·231 when the upper bottle contained water alone, and 0·229 when it was water charged with three-fourths of its volume of nitrous oxide gas,—to 1 carbonic acid remaining undiffused in the lower bottle in both cases.

It appeared, then, that the liquid diffusion of carbonic acid was a slow process compared with its gaseous diffusion, quite as much as days are to minutes.

That this diffusion of the liquid carbonic acid takes place with undiminished vigour into water already saturated with nitrous oxide, the substance of all others most resembling carbonic acid in solubility and the whole range of its physical qualities. The diffusion of the liquid carbonic acid appears no more repressed by the liquid nitrous oxide, than the diffusion of gaseous carbonic acid is by gaseous nitrous oxide.

But the chief interest of these observations was the practical solution which they give to the question, whether, in conducting experiments on liquid diffusion, accidental causes of disturbance and intermixture of two liquids, communicating freely with each other, can be avoided. It was made evident that little is to be feared from accidental dispersion when ordinary precautions are taken.

An excess of density in the lower liquid of not more than $\frac{1}{1000}$ th part is found adequate to prevent any considerable change of place of the latter,—from expansion by heat, accidental tremors and such disturbing causes, which must exist,—for days together.

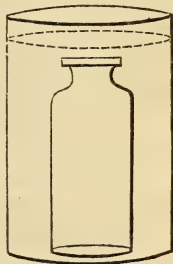
(2.) Another early inquiry was, how far is the diffusion of various salts governed or modified by the density of their solutions.

Solutions of eight hydrated acids and salts were prepared, having the common density of 1·200, and were set to diffuse into water in the following manner:—

Eighteen or twenty six-ounce phials were made use of to

contain the solutions, and to form what I shall call the Solution phials or cells. They were of the same make and selected from a large stock, of the common aperture of 1.175 inch. Both the mouths and bottoms of these phials were ground flat. The mode of making an experiment was first to fill the phial to the base of the neck, or rather to a constant distance of 0.6 inch below the ground surface of the lip. A little disc of cork, provided with a slight upright peg of wood, was then floated upon the solution in the neck, after having been first dipped in water. The neck itself was now filled up with pure water by means of a pointed sponge, the drop suspended from the sponge being made to touch the peg of the float, and water caused to flow in the gentlest manner, by slightly pressing the sponge. The only other part of the apparatus, the water-jar, was a plain cylindrical glass jar, of which the inner surface of the bottom was flat or slightly concave, to give a firm support to the phial. The phial, with its solution only, was first placed in this jar partly filled with distilled water, and the neck of the former was then filled up with distilled water in this position, as before described, to avoid any subsequent movement. The phial was ultimately entirely covered to the depth of an inch with water, which required about 30 ounces of the latter, fig. 2. The saline solution in the diffusion cell or phial thus communicated freely with about five times its volume of pure water, the liquid atmosphere which invites diffusion. Another modification of this procedure was the substitution of phials cast in a mould, of the capacity of 4 ounces, or more nearly 2080 grs., which were ground down to a uniform height of 3.8 inches. The neck was 1.25 inch in diameter and 0.5 inch in depth; and the phial was filled up with the solution to be diffused to that point. The solution cell or phial and the water-jar form together a diffusion cell.

Fig. 2.



The diffusion was stopt, after twenty-seven days in the present experiments, by closing the mouth of the phial with a plate of glass, and then raising it out of the water-jar. The quantity of salt or of acid which had found its way into the water-jar,—the diffusion product as it may be called,—was then determined by evaporating to dryness for the salts, and by neutralizing the same liquid with a normal alkaline solution for the acids. The quantities of the acids diffused are estimated at present as protohydrates for the sake of comparison with the salts.

Table I.—Diffusion of Solutions of Density 1·200. Temp. 66° Fahr.

	Placed in solution cell.		Found in water-jar.	
	Proportion of anhydrous salt, or of acid protohydrate, to 100 of water.	Boiling-point.	Diffusion product.	
			In grains.	Ratio.
Chloride of sodium.....	34·21	225·5	269·80	100
Nitric acid	37·93	227	581·20	215·42
Sulphuric acid.....	29·03	223	455·10	168·68
Chloride of potassium (density 1·178)	34·86	221	320·30	118·71
Bisulphate of potash	31·85	216	319·00	118·23
Nitrate of soda	32·42	220	260·20	96·44
Sulphate of magnesia.....	22·38	214	95·87	35·53
Sulphate of copper.....	21·56	213½	77·47	28·71

It appears that the diffusion from solutions of the same density is not equal but highly variable, ranging from 1 to 0·1333.

The results also favour the existence of a relation between large or rapid diffusibility and a high boiling-point. The latter property may be taken to indicate of itself a high degree of attraction between the salt and water.

I. CHARACTERS OF LIQUID DIFFUSION.

1. Diffusion of Chloride of Sodium.

The characters of liquid diffusion were first examined in detail in the case of this salt.

(1.) Do different proportions of chloride of sodium in solution give corresponding amounts of diffusion?

Solutions were prepared of chloride of sodium in the proportion of 100 water with 1, 2, 3 and 4 parts of the salt.

The diffusion of all the solutions was continued for the same time, eight days, at the mean temperature of 52°·5 Fahr.

Proportion of salt to 100 water.	Diffusion product.	
	In grains.	Ratio.
1	2·78	1
2	5·54	1·99
3	8·37	3·01
4	11·11	4·00

The quantities diffused appear therefore to be closely in proportion (for this salt) to the quantity of salt in the diffusing solution. The density of the solutions containing 1, 2, 3 and 4 parts of chloride of sodium, was at 60°, 1·0067, 1·0142,

1·0213, 1·0285. The increase of density corresponds very nearly with the proportion of chloride of sodium in solution. A close approach to this direct relation is indeed observable in most salts, when dissolved in proportions not exceeding 4 or 5 per cent.

The relation which appears in these results is also favourable to the accuracy of the method of experimenting pursued. The variation from the speculative result does not in any observation exceed 1 per cent.

(2.) Is the quantity of salt diffused affected by temperature?

The diffusion of similar solutions of chloride of sodium was repeated at two new temperatures, $39^{\circ}\cdot6$ and 67° , the one being above and the other below the preceding temperature. It was necessary to use artificial means to obtain the low temperature owing to the period of the season. A close box of double walls, namely the ice-safe of the Wenham Ice Company, was employed, masses of ice being laid on the floor of the box, and the water-jars supported on a shelf above. The water and solution were first cooled separately for twenty-four hours in the ice-box, before the diffusion was commenced. It was found that the temperature could be maintained within a range of 2° or 3° for eight days. It was doubtful however whether the temperature was constantly the same to a degree or two in all the jars; and the results obtained at an artificial temperature were always less concordant and sensibly inferior in precision to observations made at the atmospheric temperature.

Diffusion of Chloride of Sodium.

Proportion of salt to 100 water.		Diffusion product.	
		In grains.	Ratio.
1	At $39^{\circ}\cdot6$	2·63	1·
2	At $39^{\circ}\cdot6$	5·27	2·00
3	At $39^{\circ}\cdot6$	7·69	2·92
4	At $39^{\circ}\cdot6$	10·00	3·80
1	At 67°	3·50	1·
2	At 67°	6·89	1·97
3	At 67°	9·90	2·83
4	At 67°	13·60	3·89

The proportionality in the diffusion is still well-preserved at the different temperatures. The deviations are indeed little, if at all, greater than might be occasioned by errors of observation. The ratio of diffusion, for instance, from the solutions containing 4 parts of salt, is 3·80 and 3·89 for the two temperatures, which numbers fall little short of 4.

The diffusion manifestly increases with the temperature,

and as far as can be determined by three observations, in direct proportion to the temperature. The diffusion-product from the 4 per cent. solution increases from 10 grs. to 13·60, with a rise of temperature of $27^{\circ}\cdot4$, or rather more than one-third. Supposing the same progression continued, the diffusibility of chloride of sodium would be doubled by a rise of 84 or 85 degrees.

(3.) The progress of the diffusion of chloride of sodium in such experiments as have been narrated, was further studied by intercepting the operation after it had proceeded for different periods of 2, 4, 6 and 8 days. The solution employed was that containing 4 parts of salt to 100 water. Two of the six-ounce phials were diffused at the same time for each period. The temperature given is the mean of the temperatures of a water-jar observed each day of the period. The daily fluctuation was not more than two or three-tenths of a degree Fahr.

In 2 days, temperature $63^{\circ}\cdot7$; the salt diffused was 4·04 and 3·86 grs.; mean 3·95 grs.

In 4 days, temperature $63^{\circ}\cdot7$; the salt diffused was 6·78 and 7·12 grs.; mean 6·95 grs.

In 6 days, temperature $63^{\circ}\cdot8$; the salt diffused was 10·02 and 9·70 grs.; mean 9·86 grs.

In 8 days, temperature 64° ; the salt diffused was 13·00 and 13·25 grs.; mean 13·12 grs.

The proportion diffused in the first period of two days is given directly in the first experiments. The proper diffusion for each of the three latter periods of two days is obtained by deducting from the result of each period the result of the period which precedes it:—

Diffused in 1st two days	. .	3·95 grs.
Diffused in 2nd two days	. .	3·00 grs.
Diffused in 3rd two days	. .	2·91 grs.
Diffused in 4th two days	. .	3·26 grs.

The diffusion appears to proceed pretty uniformly, if the amount diffused in the first period of two days be excepted. Each of the phials contained at first about 108 grs. of salt, of which the maximum quantity diffused is 13·12 grs. in eight days, or $\frac{1}{8\cdot24}$ of the whole salt. Still the diffusion must necessarily follow a diminishing progression, which would be brought out by continuing the process for longer time, and appear at the earliest period in the salt of most rapid diffusion.

All the experiments which follow being made like the preceding on comparatively large volumes of solution in the phial, and for equally short periods of seven or eight days, may be looked upon as exhibiting pretty accurately the initial diffusion

of such solutions, the influence of the diminishing progression being still small. The volume of water in the water-jar is also relatively so large, that the experiment approaches to the condition of diffusion into an Unlimited Atmosphere.

2. *Diffusion of various Salts and other Substances.*

With these notions regarding the influence of temperature and proportion of salt on the amount of diffusion, an examination was next undertaken of the relative diffusibility of a variety of salts and other substances. The results of this first survey I shall state as shortly as possible, as I consider these, as well as the experiments which preceded, as of a preliminary character. The experiments were all made by means of the diffusion phials already described, namely, the six-ounce phials, and with similar manipulations.

In the following experiments, the diffusion took place at a temperature ranging from 62° to 59° , mean $60^{\circ}5$, and was continued for a period of eight days; the proportion of salt in solution to be diffused being always 20 salt to 100 water, or 1 to 5. I add as usual the density of the solutions.

Table II.—Diffusion of solutions of 20 salt to 100 water, at $60^{\circ}5$, for eight days.

Name of salt.	Density of solution at 60° .	Anhydrous salt diffused.	
		In grains.	Means.
Chloride of sodium ...	1.1265	58.5	
Chloride of sodium ...	1.1265	58.87	58.68
Sulphate of magnesia...	1.185	27.42	27.42
Nitrate of soda	1.120	52.1	
Nitrate of soda	1.120	51.02	51.56
Sulphate of water	1.108	68.79	
Sulphate of water	1.108	69.86	69.32
Crystallized cane-sugar	1.070	26.74	26.74
Fused cane-sugar	1.066	26.21	26.21
Starch-sugar (glucose)	1.061	26.94	26.94
Treacle of cane-sugar	1.069	32.55	32.55
Gum-arabic	1.060	13.24	13.24
Albumen	1.053	3.08	3.08

The following additional ratios of diffusion were obtained from similar solutions at a somewhat lower temperature, namely 48° ;—chloride of sodium 100, hydrate of potash 151.93, ammonia (from a 10 per cent. solution, saturated with chloride of sodium to increase its density) 70, alcohol (saturated with chloride of sodium) 75.74, chloride of calcium 71.23, acetate of lead 45.46.

Where two experiments upon the same salt are recorded in the table they are seen to correspond to within 1 part in 40, which may be considered as the limit of error in the present observations. It will be remarked that the diffusion of cane- and starch-sugar is sensibly equal, and double that of gum-arabic. On the other hand, the sugars have less than half the diffusibility of chloride of sodium. It is remarkable that the specifically lightest and densest solutions, those of the sugars and of sulphate of magnesia, approach each other closely in diffusibility. On comparing together, however, two substances of similar constitution, such as the two salts, chloride of sodium and sulphate of magnesia, that salt appears to be least diffusive of which the solution is densest.

But the most remarkable result is the diffusion of albumen, which is low out of all proportion when compared with saline bodies. The solution employed was the albumen of the egg, without dilution, but strained through calico and deprived of all vesicular matter. As this liquid, with a density of 1.041, contained only 14.69 parts of dry matter to 100 of water, the proportion diffused is increased in the table to that for 20 parts, to correspond with the other substances. In its natural alkaline state the albumen is least diffusive; but when neutralized by acetic acid, a slight precipitation takes place and the liquid filters more easily. The albumen is now sensibly more diffusive than before. Chloride of sodium appears 20 times more diffusible than albumen in the table, but the disparity is really greater; for nearly one-half of the matter which diffused consisted of inorganic salts. Indeed the experiment appears to promise a delicate method of proximate analysis peculiarly adapted for animal fluids. The value of this low diffusibility in retaining the serous or albuminous fluids within the blood-vessels at once suggests itself.

Similar results were obtained with egg albumen diluted and well-beaten with 1 and 2 volumes of water. The solution diluted with an equal bulk of water, and made slightly acid with acetic acid, contained $7\frac{1}{2}$ dry matter to 100 water. Diffused from two four-ounce bottles of 1.25 inch aperture, for seven days, at a mean temperature of $43^{\circ}.5$ F., it gave products of 1.73 and 1.48 gr., from the evaporation of two water-jars, in which cubic crystals of common salt were abundant. The whole matter thus diffused in two cells was found to consist of—

Coagulable albumen	0.94 gr.
Soluble salts	2.27 grs.
	3.21 grs.

The diffusion product of the same solution of albumen left alkaline, or without the addition of acetic acid, in the same

circumstances, was 1·41 and 1·20 grs. in two cells; and consisted of—

Coagulable albumen . . .	0·63 gr.
Soluble salts	1·98 gr.
	<u>2·61 grs.</u>

The diffusion product of a solution of $7\frac{1}{2}$ parts of chloride of sodium to 100 water, from similar cells and for the same time and temperature, would amount to about 30 grs. of salt. It is to be remarked also that 5·53 grs. of the ignited salt diffused from albumen contained 1·32 gr. of potash or 23·9 per cent., which is a high proportion, and indicates that salts of potash diffuse out more freely from albumen than salts of soda.

Nor does albumen impair the diffusion of salts dissolved together with it in the same solution, although the liquid retains its viscosity. Three other substances, added separately in the proportion 5 parts to 100 of the undiluted solution of egg albumen, were found to diffuse out quite as freely from that liquid as they did from an equal volume of pure water: these were chloride of sodium, urea and sugar. Urea proved to be a highly diffusible substance. It nearly coincided in rate with chloride of sodium.

A second series of salts were diffused containing 1 part of salt to 10 of water; a smaller proportion of salt which admits of the comparison of a greater variety of salts. The temperature during the period of eight days was remarkably uniform, 60° — 59° .

Table III.—Diffusion of solutions of 10 salt to 100 water at $59^{\circ}\cdot 5$.

Name of salt.	Density of solution at 60° .	Anhydrous salt diffused.	
		In grains.	Means.
Chloride of sodium.....	1·0668	32·3	
Chloride of sodium.....	1·0668	32·2	32·25
Nitrate of soda	1·0622	30·7	30·7
Chloride of potassium	1·0596	40·15	40·15
Chloride of ammonium	1·0280	40 20	40·20
Nitrate of potash	1·0589	35·1	
Nitrate of potash	1·0589	36·0	35·55
Nitrate of ammonia ...	1·0382	35·3	35·3
Iodide of potassium ...	1·0673	37·0	37·0
Chloride of barium ...	1·0858	27·0	27·0
Sulphate of water	1·0576	37·18	
Sulphate of water	1·0576	36·53	36·85
Sulphate of magnesia...	1·0965	15·3	
Sulphate of magnesia...	1·0965	15·6	15·45
Sulphate of zinc.....	1·0984	15·6	
Sulphate of zinc.....	1·0984	16·0	15·80

Before advertng to the relations in diffusibility which appear to exist between certain salts in the preceding table, I may state the results of the diffusion of the same solutions at a lower temperature.

Table IV.—Diffusion of solutions of 10 salt to 100 water at 37°·5.

Name of salt.	Anhydrous salt diffused.	
	In grains.	Means.
Chloride of sodium	22·21	
Chloride of sodium	22·74	22·47
Nitrate of soda	22·53	
Nitrate of soda	23·05	22·79
Chloride of ammonium	31·14	31·14
Nitrate of potash	28·84	
Nitrate of potash	28·56	28·70
Nitrate of ammonia	29·19	29·19
Iodide of potassium	28·10	28·10
Chloride of barium	21·42	21·42
Sulphate of water	31·11	
Sulphate of water	28·60	29·85
Sulphate of magnesia	13·03	
Sulphate of magnesia	13·11	13·07
Sulphate of zinc.....	11·87	
Sulphate of zinc.....	13·33	12·60

The near equality of the quantities diffused of certain isomorphous salts is striking at both temperatures. Chloride of potassium and chloride of ammonium give 40°·15 and 40°·20 grs. respectively in the first table. Nitrate of potash and nitrate of ammonia 35·55 (mean) and 35°·3 grs. respectively in the first table, and 28·70 and 29·19 grs. in the second table. Sulphate of magnesia and sulphate of zinc 15·45 and 15·8 grs. (means) in the first table, with 13·07 and 12·60 grs. in the second. The relation observed is the more remarkable, that it is that of equal weights of the salts diffused, and not of atomically equivalent weights. In the salts of ammonia and potash, this equality of diffusion is exhibited also, notwithstanding considerable differences in density between their solutions; the density of the solution of chloride of ammonium, for instance, being 1·0280 and that of chloride of potassium 1·0596. It may have some relation however, but not a simple one, to the density of the solutions; sulphate of magnesia, of which the solution is most dense, being most slowly diffusive; and salts of soda being slower, as they are generally denser in solution, than the corresponding salts of potash. Nor does it depend

upon equal solubility, for in none of the pairs is there any approach to equality in that respect.

A comparison was now made of the diffusibility of several acids. They were diffused from the same six-ounce phials, and for eight days. Solutions were prepared in the proportion of 4 parts of the anhydrous acid to 100 parts of water. The quantity of acid which diffused into the water-jar was estimated by the proportion of carbonate of soda which it neutralized.

Table V.—Diffusion of acid solutions (4 acid to 100 water) at 59°3.

Name of acid.	Density of solution at 69°.	Anhydrous acid diffused.	
		In grains.	Means.
Nitric acid	1·0243	29·21 28·19	28·7
Hydrochloric acid	1·0225	34·22 33·99	34·1
Sulphuric acid	1·0317	18·71 18·26	18·48
Acetic acid.....	1·0094	19·13 17·19	18·16
Oxalic acid.....	1·0235	12·38 12·38	12·38
Arsenic acid	1·0320	12·16 12·16	12·16
Tartaric acid	1·0194	9·90 9·69	9·79
Phosphoric acid.....	1·0284	9·09 9·09	9·09
Chloride of sodium	1·0285	12·32	12·32

Considerable latitude thus appears to exist in the diffusibility of the different acids. To make the result for nitric acid fairly comparable with that for hydrochloric acid, the former should be increased in the proportion of 54 to 63, that is estimated as nitrate of water. This calculation gives 33·5 grs. of nitrate of water diffused, which approaches closely to 34·1 grs., the quantity for chloride of hydrogen or hydrochloric acid. The quantity of soda neutralized by the sulphuric and hydrochloric acids diffused was as 14·32 to 28·97, or nearly as 1 to 2. Sulphuric and acetic acids, on the other hand, appear to be equally diffusible. Phosphoric acid is the least diffusible acid in the series, presenting only about half the diffusion product of the two last-mentioned acids. The solution of phosphoric acid had been boiled for half an hour before diffusion, and was therefore in the tribasic state. The same precaution was not thought of for arsenic acid, although it is possibly required by this acid also. These two acids do not exhibit the equality of diffusion anticipated from their recognized isomorphism, but it is to be stated that the acidi-

metrical method of analysis followed is not so properly applicable to these two acids as it is to all the others.

3. *Diffusion of Ammoniated Salts of Copper.*

It was interesting to compare together such related salts as sulphate of copper, the ammoniated sulphate of copper or soluble compound of sulphate of copper with 2 equivs. of ammonia and the sulphate of ammonia. It is well known that metallic oxides, or subsalts of metallic oxides, when dissolved in ammonia or the fixed alkalies, are easily taken down by animal charcoal. This does not happen with the ordinary neutral salts of the same acids, which are held in solution by a strong attraction. Supposing the existence of a scale of the solvent attraction of water, the preponderance of the charcoal attraction will mark a term in that scale. And if the solvent force is nothing more than the diffusive tendency, it will follow that salts which can be taken down by charcoal must be less diffusible than those which cannot.

Of sulphate of ammonia and sulphate of copper, solutions were prepared, consisting of 4 anhydrous salt to 100 water, the sulphate of ammonia being of course taken as NH_4O , SO_3 . The solution of the copper salt was divided into two portions, one of which had caustic ammonia added to it in slight excess, so as to produce the azure blue solution of ammonio-sulphate of copper.

The solutions were diffused for eight days, at a mean temperature of $64^{\circ}\cdot 9$ for the sulphates and nitrates, and $67^{\circ}\cdot 7$ for the chlorides.

Table VI.—Diffusion of solutions, 4 salt to 100 water.

Name of salt.	Density of solution at temperature of experiment.	Anhydrous salt diffused in grains.
Sulphate of ammonia.....	1·0235	12·13
Sulphate of ammonia	1·0235	11·96
Sulphate of copper	1·0369	6·19
Sulphate of copper	1·0369	6·51
Ammonio-sulphate of copper	1·0308	1·45
Ammonio-sulphate of copper	1·0308	1·43
Nitrate of ammonia	1·0136	16·15
Nitrate of ammonia	1·0136	15·44
Nitrate of copper	1·0323	9·77
Nitrate of copper	1·0323	9·77
Ammonio-nitrate of copper	1·0228	1·77
Ammonio-nitrate of copper	1·0228	1·36
Chloride of ammonium	1·0100	16·18
Chloride of ammonium	1·0100	17·00
Chloride of copper.....	1·0328	10·83
Chloride of copper.....	1·0328	10·48
Ammonio-chloride of copper.....	1·0209	4·54
Ammonio-chloride of copper	1·0209	3·94

It is to be observed, that in preparing the ammoniated salts, the solutions of the neutral salts of copper were slightly diluted by the water of the solution of ammonia added to them, so that the proportion of salt of copper which they possessed was sensibly reduced below 4 per cent. On the other hand, the copper salt which diffused out is estimated, not as ammoniated, but as neutral salt. It will be observed that the quantity of sulphate of copper diffused out in the experiments falls from 6.35 in the neutral salt to 1.44 gr. in the ammoniated salt; of nitrate of copper from 9.77 to 1.56, and of chloride of copper from 10.65 to 4.24. These numbers are to be taken only as approximations; they are sufficient however to prove a much reduced diffusibility in the ammoniated salts of copper.

It will be remarked that the nitrate of ammonia and chloride of ammonium approximate, 15.80 and 16.59 grs.; as do also the nitrate and chloride of copper, 9.77 and 10.65 grs.; the chlorides, which were diffused at the higher temperature by 2°.8, exceeding the nitrates in both cases.

4. Diffusion of Mixed Salts.

When two salts can be mixed without combining, it is to be expected that they will diffuse separately and independently of each other, each salt following its special rate of diffusion.

(1.) Anhydrous sulphate of magnesia and sulphate of water (oil of vitriol), one part of each, were dissolved together in 10 parts of water, and the solution allowed to diffuse for four days at 61°.5.

The water-jar was found to have acquired—

Sulphate of magnesia . . .	5.60 grs.
Sulphate of water . . .	21.92 grs.
	27.52 grs.

The experiment with the same diffusion cell and liquid being continued for a second period, this time of eight days, there was found to be simultaneously diffused, of—

Sulphate of magnesia . . .	9.46 grs.
Sulphate of water . . .	29.32 grs.
	38.78 grs.

It is obvious that the inequality should be greatest in the first period of diffusion, or with the initial diffusion, as it actually appears above, and become less and less sensible as the proportion of the low diffusive salt comes to be increased in the solution phial.

In former experiments upon the solution of sulphate of

magnesia alone in water, as 1 salt to 10 water, compared with sulphate of water, also as 1 to 10, the disparity in the diffusion of these two salts was less considerable, being only as 1 to 2·385, instead of 1 to 3 or 4.

(2.) A solution was also diffused of 1 part of anhydrous sulphate of soda and 1 part of chloride of sodium in 10 parts of water, for four days at $61^{\circ}5$. The salt which diffused out in that time consisted of—

Sulphate of soda	9·48 grs.
Chloride of sodium	17·80 grs.
	27·28 grs.

The sulphate of soda in the last experiment had begun to crystallize in the solution phial, from a slight fall of temperature, before the diffusion was interrupted, a circumstance which may have contributed to increase the inequality of the proportions diffused of these two salts.

(3.) A solution of equal weights of anhydrous carbonate of soda and chloride of sodium, namely, of 4 parts of the one salt and 4 parts of the other, to 100 water, was diffused from 3 four-ounce phials of 1·25 inch aperture, at a mean temperature of $57^{\circ}9$ and for seven days. The diffusion product amounted to 17·10, 17·58 and 18·13 grs. of mixed salt in the three experiments. The analysis of the last product of 18·13 grs. gave—

Carbonate of soda	5·68	31·33
Chloride of sodium	12·45	68·67
	18·13	100·00

Here the carbonate of soda presents a diffusion less than one-half of that of chloride of sodium. The difference is again greater than the peculiar diffusibilities of the same salts as they appear when the salts are separately diffused. For in experiments made in the same phials with solutions of 4 parts of each salt singly to 100 water, but with a lower temperature by $3^{\circ}6$, namely, at $54^{\circ}3$, the diffusion product of the carbonate of soda was 7·17 and 7·34 grs. in two experiments, of which the mean is 7·25 grs.; while the diffusion product of the chloride of sodium was 11·18 and 10·73 grs. in two experiments, of which the mean is 10·95 grs. The quantity of chloride of sodium diffused being taken at 100 in both sets of experiments, we have diffused—

Of carbonate of soda 66·18, when diffused singly.

Of carbonate of soda 45·64, when diffused with chloride of sodium.

The least soluble of the two salts appears in all cases to have its diffusibility lessened in the mixed state. The tendency to crystallization of the least soluble salt must evidently be increased by the admixture. Now it is this tendency, or perhaps more generally the increased attraction of the particles of a salt for each other, when approximated by concentration, which most resists the diffusion of a salt, and appears to weaken the diffusive force in mixtures, as it is also found to do so in a strong solution of a single salt.

(4.) Equal weights of nitrates of potash and ammonia dissolved, as in certain preceding experiments, in five times the weight of the mixed salts of water, and diffused for eight days, gave in two experiments—

	At 59°·4.	At 52° 6.
Nitrate of potash . . .	28·39	25·88
Nitrate of ammonia . . .	36·16	30·36
	64·55	56·24

The inequality in the diffusion of these two nitrates is singular, considering that in solutions of 1 salt to 10 water, they appeared before to be equally diffusive. But on now comparing the diffusion of solutions of 1 salt to 5 water, at 52°·6, the salts no longer diffused in equal proportions:—

Nitrate of potash gave .	57·93 grs.
Nitrate of ammonia gave .	82·08 grs.

The solution of nitrate of potash last diffused was nearly a saturated one, while that of nitrate of ammonia is far from being so. The first has its diffusibility, in consequence, impaired, and falls considerably below the second.

The relatively diminished diffusibility of sulphate of magnesia, when associated with sulphate of water, is probably connected with a similar circumstance; sulphate of magnesia being less soluble in dilute sulphuric acid than in pure water.

(5.) The salt which diffused from a strong solution of sulphates of zinc and magnesia, consisting of 1 part of each of these salts in the anhydrous state and 6 parts of water, did not consist of the two salts in exactly equal proportions. The mixture of salts, diffused for eight days, as in the late experiments, gave the following results:—

	Exp. I.	II.	III.
Sulphate of zinc . . .	8·12	7·49	8·12
Sulphate of magnesia .	8·68	8·60	8·75
	16·80	16·09	16·87

There is therefore always a slight but decided preponderance of sulphate of magnesia, the more soluble salt, in the

diffusion product. These last experiments were made at an early period with another object in view, namely, to ascertain whether in closely related salts, such as the present sulphates of magnesia and zinc, the two salts might be elastic to each other, like the particles of one and the same salt, so that one salt might possibly suppress the diffusion of the other, and diffuse alone for both. The experiments lend no support to such an idea.

It appears from all the preceding experiments, that the inequality of diffusion which existed, is not diminished but exaggerated in mixtures, a curious circumstance, which has also been observed of mixed gases.

[To be continued.]

XXIV. *Geometry and Geometers.*

*Collected by T. S. DAVIES, Esq., F.R.S. and F.S.A.**

No. VI.

AS I have had frequent occasion to speak of Dr. Matthew Stewart in the earlier portion of these papers, it will be proper here to make one statement more.

From the character of his General Theorems, I have long entertained the opinion that Dr. Stewart had discovered a considerable number of Porisms besides those printed in his book, and the two he gave to Dr. Simson. The description of his MSS. given by Playfair, in his memoir of that illustrious geometer, led me to believe that it would be possible to make out not only the propositions but likewise their demonstrations, somewhat in the manner that I did with the Porismatic part of his General Theorems in the Edinburgh Transactions a few years ago. I consequently applied to an eminent mathematical archæologist to obtain for me information as to what had become of those papers, and whether they were accessible for such a purpose. In a short time he sent me the copy of a letter from the proper custodian of the papers, decisive on this head. *They are all destroyed—deliberately burnt; and not only his, but likewise all the MSS. of his son Dugald Stewart.* Into the motives for this act, which its perpetrator offers, I will not enter; and shall only state that he was a descendant of those two men whose writings he has thus irretrievably destroyed. I have thought it desirable to put upon record this fact: for though public indignation will not restore the lost treasures, it may prevent others from imitating the incendiary.

A great portion of the correspondence with Nourse and

* Communicated by the Author.

his successor Wingrave is of the most ordinary business character. This might be expected. A few passages even in the dreary list of £ s. d. are not without interest.

For instance, *the price of authorship*:—Mr. John Landen thus writes (Aug. 28, 1758) to Nourse, after describing his “Residual Analysis:”—

“I would have it very elegantly printed in quarto, (with Wood or Tin Cuts) upon such paper and with such letter as my *Lucubrations*. If you chuse to purchase the copy you shall have it for less than I would take of any other person.

.....

“The subject is very interesting, and I p ... ne [illegible—presume?] a considerable number will be speedily sold; therefore expect you will not give me less than two Guineas per sheet. However I shall leave it to you to pay me according as it shall sell.”

It proved here, as it often does, that an author is himself the very worst judge of what will “sell.” Nobody but systematic collectors of classes of books, knows anything of Landen’s *Discourse on the Residual Analysis*, except accidentally by mere name. The philosophy of Landen “never took;” and the truths delivered, or professedly deduced by means of it, were neither new nor in any way remarkable. Still the book was not without merit; nor the author destitute of very high mathematical powers. He was not, however, deficient in the *amour propre*; but, on the contrary, was remarkable for carrying out the principle to an extreme degree.

Dr. Gregory (who from being “bred and born” in the neighbourhood of Peterborough was likely to be well acquainted with the gossip of the place) has informed me that Landen was a man of “imposing presence and imperious manners.” He was steward to Earl Fitzwilliam, for that nobleman’s Northamptonshire estates. The then countess, who appears to have much disliked the bearing of the steward, described an interview between him and her liege lord, as that “between Lord Landen and his steward Mr. Fitzwilliam.”

Landen was perhaps the only non-academic mathematician F.R.S., who did not join with Horsley, Hutton, and the other seceders from the Royal Society on the accession of Sir Joseph Banks to the chair. Philosophy in the Society had then degenerated into Faction: a sort of scientific imitation of Whig-and-Toryism. The contest was one of partisanship, and it was conducted in the true spirit of political party. The naturalists have censured the mathematicians, and the mathematicians the naturalists, for their conduct in that discreditable dispute. The question has descended even to our

own day; and it has very recently been mooted in the *Philosophical Magazine*. It was a scene which showed undeniably the decadence of the *philosophic spirit* in the Society, rather than the overriding of one branch of science by another; although it has been almost uniformly represented that the "mathematical sciences were ousted from the Society by the overwhelming influence of the naturalists." The terms themselves were *mere symbols of party*: but we are not to assume that mathematical science was excluded from the Society because Dr. Horsley was foiled in his aspirations for the Chair, and Dr. Hutton divested of the Foreign Secretaryship. The "little band" overrated their influence in the Society; and the time is come when some definite idea of their mathematical powers and pretensions can be formed, quite independently of factious prejudices. It would be well, therefore, to judge the question apart from all party considerations.

That Horsley had as good a claim to the Chair as Banks, there is no doubt: but he attempted to carry his purpose on fictitious grounds—as the representative of the mathematical section of the Society. That his claims, however, were not overwhelming, but only *comparative with those of his competitor*, no mathematician will now venture to assert. Every work he published is "completely shelved," and no one, I believe, reached a second edition. His *name* indeed is only remembered in scientific circles by his connexion with these unhappy disputes. That his supporters and fellow-seceders were so many Newtons and Halleys, who will assert, even though the names of Maskelyne, Maseres and Hutton were on that list? Waring, Milner, Landen and others, kept aloof from all share in such a partisan-system of enforcing the superiority of the mathematical sciences over those of observation.

So much has been said on the other side that it does not become me to speak upon it. I am no judge of the scientific merits of the actors in it. Of the long period of "misrule" which followed, I have only to say that something of the kind might have been expected: the reign of "naturalism" was the reign of actual conquest—the conquest of a faction bearing one symbol over another faction bearing another symbol. Perhaps the political condition of Ireland at this moment is only the same history on a larger scale.

I have been tempted into this long digression, from the consideration that it is high time that disputes of so long standing should be looked at apart from the symbols of the respective parties—symbols to which the actors on either side had little claim. It is certainly absurd enough that because

two sets of aspiring parties quarreled "over a bone" some eighty years ago, that men who cultivate two very difficult and ever-expanding sciences, should *now* look upon each other with jealousy, because those two factions assumed the names of these two sciences as the symbols of their factions. Even as regards the suppression or publication of papers in the *Philosophical Transactions*, it will be more frequently found that any impropriety has arisen from the influence of persons pursuing the same science than the opposite one—at any rate the alleged impropriety.

Another writer proposes to Nourse to take a work on his own hands, under the title of *Syntagma Analyseos: or a New Introduction to the Mathematics*. After a laudatory account of himself, he gives the contents, and concludes with the following:—

"As I often write to the Diaries, Magazines &c. under various fictitious appellations, I may thereby forward its sale by recommending or quoting it."

The method of indirect puffing was not unknown even then! Till I met with this letter, however, I had taken the name of the writer (Malachy Hitchins, Exeter College, Oxford) to be itself fictitious. His contributions (at least under his own name) are respectable for the time, though none of them bespeak powers far above mediocrity. I do not think that Nourse was taken with the bait of his "recommending" his own book; and indeed I have no knowledge of its having been published at all.

The following passage is only curious as showing a mathematician's notions of *amusement*. It is from a printed proposal to publish by subscription a "Complete Course of Mathematics" in 96 sixpenny numbers; the scheme of which was abandoned, and the MS. offered to Nourse, who also appears to have declined it.

"Even to those who peruse books for amusement, the author ventures to recommend this work, presuming that more real entertainment, much more genuine satisfaction must flow from it, than can arise from an insignificant romance or fictitious tale, which serve chiefly to vitiate the taste and corrupt the morals."

This is dated Newcastle 1770, and the author is John Davidson—a mathematician of great local note, and of some general reputation in those days.

A letter from Mr. Andrew Marshall, dated Dover, Oct. 9, 1773, contains a proposal to Nourse to take a translation of the first three books of *Simsoni Sect. Con.*, or a complete translation of the whole work, if Nourse should prefer it. The letter is valuable in one respect, as giving a reason for this proposal.

“It was at the request of Dr. Matthew Stewart and Dr. Williamson professor of Mathematics in the University of Glasgow, that I undertook that translation: and the reason why they interested themselves in it was, that they thought their colleges were not so well attended, while the students were obliged to read a latin book on so abstruse a subject, as they would be, was the text book in English and at a lower price.”

The translation—at least so I judge, but at any rate a translation—was published in Edinburgh by Charles Elliott, in 1775. Nourse’s name does not appear among the London publishers on the title-page. It had been well for English science if all the books that Simson and Stewart ever wrote had been printed in our own language.

Trinity College, Dublin, has been, perhaps, the most tenacious adherent to the use of Latin in its lectures, exercises and responsions. Amongst the works published for the use of that college was Dr. Hugh Hamilton’s *Sectiones Conicæ*, 4to, 1758, in Latin. This work, however, though compulsorily read in college, was otherwise slow in its sale—much slower indeed than its great merit would have led us to expect. In a letter to Nourse in 1768, he says, “there were 600 copies printed, and about 100 of them remained” then in Dublin; whilst he speaks of 230 copies in the hands of Johnston, the London publisher. These last, with the copper-plates and copyright, he offers to Nourse, and ultimately appears to have sold them to him, with all else relating to the work, for two shillings per volume, though he says the print alone cost him more than three shillings per copy. He adds:—

“You were certainly right in supposing that the treatise would have sold much better had it been written in English, for Johnston told me two years after it was published that he was sure he might in that time have sold almost the whole impression had it not been in latin. This makes me imagine that when you get the *Property of Work* (and the copperplates which cost me 20£) it may be worth your while to publish a Translation sometime hence, a Person of tolerable skill would translate such a Book as easily as he could transcribe it.”

He then goes on to mention the additions he would make

to it, in the form of an appendix, when translated. This, however, in a subsequent letter he proposes to replace by a few occasional scholia. Nourse appears to have sent him the translation in MS.; which, though so easy to make, he considered to be very faulty, and an amended translation in its turn was only somewhat less faulty. It ended in two Trinity men, who were reading for fellowships, recommended by Dr. Hamilton, being employed. Their remuneration was *ten guineas*!

The following is one of the most singular facts, perhaps, in Irish Church history:—

“Let him [his friend Matthew Raper] know I would write to him had I any thing worth communicating, further than one thing (which I know his friendship to me will make him pleased with) that I have *very fortunately and without any solicitation of mine got the Deanery of Armagh, the best preferment in our Church under a Bishoprick and equal in value to some of them.*” (Letter, Ap. 30, 1768.)

The worthy Dean views his good fortune under a sufficiently worldly aspect: and a fair share of worldly tact is shown in the following extract from another of his letters (Nov. 23, 1772):—

“It is proper to observe to you that the Euclid to which my citations refer is Whiston’s latin edition of Tacquet’s Euclid. And I think it will be proper to apprise the reader of this at the end of the Translator’s Preface, for I am of your opinion that such a preface will be absolutely necessary, since you cannot avoid saying in the Titlepage, *Translated from the Latin.* I think you or any one that is acquainted with the work might very easily write such a preface; as nothing more would be necessary than to say that the following Treatise written in Latin was published at such a time and has since been *so well received by the learned that the Professors in the several universities in England and Ireland* have used and recommended it in preference to all others on the same Subject, and therefore the *Translator* thought that an English Edition would be an acceptable Present to the Public in a Country where so many had distinguished themselves by their great proficiency in mathematical studies tho they had not much cultivated the learned languages. He might then (*to show he had read other works of this kind*) mention some of the Particulars in which he thought *this work had the advantage of others.* He might say some thing in general of the Method or plan upon which the Author proceeds, and his manner of executing it, and then for a further account of the method refer the Reader to the Author’s Preface which follows.

“I have only mentioned these hints to show what a Preface of this sort usually contains and that there can be no great difficulty in drawing it up. And I dare say if you were to write to Mr. Williamson or any other Teacher who has read the Book and communi-

cate these hints *as your own thoughts*, and request his assistance he would very soon draw up a Preface to your purpose, and you might have it in your power to *oblige him again*."

"Secrets in all trades *but mine* !" The learned author and the liberal bookseller combine to represent a mere speculation of their own as the act and judgement of the "translator,"—that invisible, if not imaginary, personage of all ages as well as our own.

It does not appear who did draw up the "translator's preface:" but it is executed pretty closely in accordance with the above prescription; and some changes are introduced into the text as suggested by Dr. Hamilton, which form real improvements upon an already valuable treatise.

It seems somewhat strange that Dr. Abram Robertson should print a ponderous 4to in seven books, only a few years after (1792), *also in Latin*, and very much on the same plan as those of Simson and Hamilton, though more closely imitating the latter. The necessity that was felt, even by authors and publishers, for putting the scientific works they issued in a language which could be read by *all*, had produced no influence at Oxford; but even *there* it was ultimately felt to be absolutely necessary to give an English edition of at least a part of the work, which was accordingly done in 8vo a few years afterwards.

Even Euclid was recently read in Latin in the Dublin College; but as it has since been translated, I presume it is now* read there in English. Dr. Elrington's edition, which is there used, differs a good deal from Simson's, but most of all in the treatment of proportion: but I only refer to it here as another instance of the paramount necessity for writing all books on science in our vernacular tongue.

By some letters from the Rev. Francis Holliday, Rector of West Marsham, Notts, it appears that the price paid for the copyright of that gentleman's *Fluxions* was twenty-three guineas; that is, *one guinea per sheet*. (Letter to Nourse, con-

* Since this was written, a Dublin friend whom I had asked about the *motive* for the retention of Latin as the medium of so much of the Trinity College exercises, "wonders where I could have met with so antiquated a thing as a Latin Euclid used in the College." My own copy is marked "editio quarta, 1813." The Latin appears to be still the medium of examination for the junior fellowships; though (judging from the Dublin University Calendar) dispensed with in the degree examinations. This traditionary practice is hence gradually "wearing out" even in its last stronghold; and certainly in the case of fellowship examinations, less exception can be taken to the practice, than in the case of degrees, whether in honours or not.

taining a statement of accounts, Nov. 22, 1777.) He asked thirty guineas, and estimated it at seventeen or eighteen sheets; see letter, Sept. 26, 1776. Fluxions were cheap then, but they command no price at all now.

It may be worth noticing, that Dealtry's is the last book published with the name and notation of Fluxions in this country; but Jephson's the last with the name only, having the notation of Leibnitz instead of Newton's; the former in two successive editions, and the latter in two volumes issued at different times. The eleventh edition of Hutton's Course is, however, the last English book in which either the name or notation appeared (1835, 1837); and though I strongly urged upon Dr. Gregory, who acted as principal editor of that edition, the necessity for a change, I failed to convince him. This was the more inexplicable to me, as he had for many years admitted the language and notation of the Differential Calculus (though, perhaps, not its metaphysics) into the Ladies' Diary, of which he was the editor. It first appears in 1824 in the Diary*. His argument for retaining it in Hutton's Course was, that Dr. Hutton himself would have insisted on its retention; and he felt himself bound in honour to make no further changes in that work than the author himself would have made under the same circumstances, and with a full knowledge of the state of mathematical science in 1836. As I am personally interested in this question, I may be allowed to state that I consider that view to have been a mistaken one; and that a resolute adherence to what it was supposed Dr. Hutton would have done, has driven the work "out of the market." The ultimate changes made in it came too late; and yet no man was more eminently qualified to make them than Dr. Gregory.

It appears from letters written in 1811 to Wingrave (successor to Nourse) by Miss Maskelyne, that *the Greenwich observations were the private property of Dr. Maskelyne*. They are claimed as such; and Sir Joseph Banks's authority is quoted in support of it. Yet the Board of Admiralty paid

* See two interesting papers on the Introduction of the Notation of the Differential Calculus into this Country by an eminent *anonymé*, and by Mr. Wilkinson, in the *Mechanics' Magazine* for 1849. It may, however, be remarked, that Dr. Gregory introduced the differential notation into his Trigonometry (using the δ instead of the d) as far back as 1816; and that to the edition of Hutton's Course of (vol. ii.) 1837, he gave a translation, literally, of a portion of Lubbe's treatise. I am not able at this moment to give the dates of either Dealtry's or Jephson's works; but the former ranged somewhere from 1812 to 1814, and the latter a little before and after 1820.

for paper, print, instruments, and a salary for making those observations! Many strange affairs, however, have occurred with respect to books printed by the Admiralty; to some of which I may hereafter direct more particular attention—relating, of course, to “by-gone days.”

It seems that even 80 or 100 years ago the booksellers published a good deal “on commission” for the authors: but they do not seem to have carried this kind of business to the extent that we see it in our day, nor to have charged quite so heavily for their services as we now find to be the case. Nor do they appear to have been so eager for that kind of business as their successors have become.

A Mr. John Wright of Edinburgh, a friend of Marshall’s, sent 100 copies of a work on Trigonometry (intended as a supplement to Simson’s Euclid) to Nourse in 1772 for sale. In 1783 it appears that copies to the amount of six guineas had been sold; and that the “charges” against this were three pounds seventeen shillings!

Many large works that have appeared were first published by public subscription. Usher’s Astronomy, Vince’s Astronomy, Horsley’s Newton*, Taylor’s Tables, and some others, are matters relative to which there is more or less correspondence in this mass of papers. A memorandum by Wingrave respecting one work, the name of which I cannot decipher, is “not enough sold to pay the advertisements.”

Several papers of Emerson’s occur amongst Mr. Maynard’s collection, but none of them of much scientific importance; and, indeed, all that is of any value was subsequently incorporated with his published works. His books are even now ubiquitous, and his name is familiar to every tongue; and hence quotation would be idly superfluous. In fact, but for the purpose of correcting a very general popular error with respect to him, his name might have been altogether omitted from these papers.

Emerson wrote nearly twenty works in all, and upon all subjects, into the service of which such mathematics as he possessed could by any contrivance be pressed—from arithmetic to increments, fluxions, mechanics, architecture, music

* It appears from a printed list that Horsley had obtained 369 subscribers: crowned heads, nobility, personages in high civil and diplomatic offices, university and college libraries—in short, the *élite* of Europe. A less pompous and less pretending editor (though as competent to the undertaking, as Horsley was confessedly incompetent) must have sought his patrons elsewhere, and have been satisfied with a smaller number of them.

and chronology. A few of his works (and few only) display a certain amount of rough invention: but he was singularly confused in his development of a process, and one of the most uncouth of all the mathematical writers of this country. It is very probable that, abating the Method of Increments (which owed its value to its being then the only one in our language, Dr. Brook Taylor's being in Latin and untranslated), his most useful works have been the *Elements of Geometry* and the *Conic Sections*. The former contains a few theorems which are, as far as I know, original*; and the latter is va-

* It is often extremely difficult to decide respecting originality in elementary investigations; for no one can undertake to look carefully through every elementary book that has been published, to ascertain whether some particular and simple proposition might not possibly be contained in it. Nevertheless some general criteria might be laid down, which would contribute towards probability on one side or another in most cases; and this presumed probability would often limit the trouble of the search to very narrow bounds. Almost every proposition naturally refers itself to a class; and if once observed, others of that class must soon follow. If, then, upon our observing such a proposition we find it isolated, it is highly probable that it originated with the author who there gave it, or at least not long before. In the few cases which I have had occasion to examine minutely and carefully, I have rarely found this rule to fail—indeed, in no one to signify fail. This, too, is precisely the same in respect to analytical devices—and not widely different is the testimony of the history of experimental science.

This remark is made in consequence of a property of the triangle, now universally known, which appears to have been first given in an *elementary treatise* by Emerson (*Geom.*, b. ii. pr. 32, 1763). "The perpendiculars from the angular points of a triangle to the opposite sides, pass through the same point." The property was, however, enunciated by Mr. Thomas Moss, an exciseman and able geometer, in 1751; and two neat demonstrations given to it shortly after by a writer who signs ΣΟΦΟΣ (probably Simpson), and by Edward Rollinson, in Turner's *Mathematical Exercises*. A property still more general had been given four or five years previously in the *Mathematician*, edited by Rollinson. The less general property was not, however, perceived to be deducible from the more general one, and both passed without further remark than the mere solutions till attention was called to the system of connected inquiries in the *Mathematical Repository* (vol. vi.), under one aspect; and under another in the *Phil. Mag.*, vol. ii. p. 26, 2nd Ser., and the appendix to the *Ladies' Diary*, 1835.

It may seem strange that so simple a property, and so many others similar or related to it, should have been unobserved by the ancients, and by their earlier followers after the revival of letters. It must be recollected, however, that the Greek geometers only valued a theorem (or even a Porism) except so far as it contributed to the solution of a problem. There are no traces, nor even intimations, of their having regularly attempted to form classed collections of theorems, or to arrange systematically the many beautiful series of properties of figures that could not fail to have presented themselves during the solution of problems. Such properties were only selected as would be actually required in demonstrating the constructions of the cases of a problem. Of these the seventh book of Pappus is a collection of instances. The properties of the *ἀρβηλον* given by him, form

luable for the great number of properties which the author has brought together, of which a fair portion were original. *As geometry*, however, both works are extremely impure, every geometrical difficulty being unceremoniously got over by an algebraical equation—a practice only too common amongst the so-called geometrical writers of our own time. This, however, is not the geometry bequeathed us by the Greeks, and exemplified by the Andersons, the Gregories, the Halleys, the Simsons, and the Stewarts of these isles.

The popular error to which I referred is, that Emerson was the Coryphæus of the non-academic class of geometers. He has never been recognized by themselves as the head, the founder, or the leader of their class; but I suppose the frequency of his books on the stalls has led men little acquainted with the history of English geometry to infer that they either are, or have been, in great demand, and consulted as oracles. This never-ending reappearance is more due to the almost indestructible paper on which they were printed, and the firm bindings in which they were issued, than to any other cause; as very few of them ever reached a second edition, and a great number lay on the bookseller's hands at the time of Emerson's death. They were pushed into notice by the per-

almost the only marked exception—or perhaps also Euclid's Porisms. We find at all events, extremely little (if anything, properly speaking) concerning lines meeting in a point, or points ranging in a straight line. The modern French geometers were the first to enter upon this class of researches with any degree of system; and the results have justified their expectations, however sanguine. We need not then, after all, feel much surprise at finding the proposition in question claiming so recent a place in geometry; and the same may be said of a great number of now-familiar truths.

Postscript, August 24.—Whilst reading the proof sheets, the *Mechanics' Magazine* of this date reached me, containing one of Mr. Wilkinson's able and elaborate analyses of our English mathematical periodicals, viz. of the *Miscellanea Curiosa Mathematica*, 1745–53, edited by Holliday, whose name has been already mentioned. As there is one passage which renders a slight modification of the preceding paragraph necessary, I quote it as it stands—from its offering less trouble, both to myself and the printer at the last moment, than recomposition and resetting would do.

Speaking of art. xxxix., “A new Proposition in Geometry demonstrated, by Mr. William Chapple,” he says:—

“This proposition is the now well-known property, that ‘the three perpendiculars of any triangle intersect in the same point,’ and although taken for granted in the solutions of Quest. 45 *Gentleman's Diary* for 1743–44; Quest. 260 *Ladies' Diary*, 1745–46, the honour of a formal enunciation and demonstration appears to be due to Mr. Chapple. The property is stated both for the acute and obtuse-angled triangle, ‘the same demonstration serving for both, which however is not conducted in so purely geometrical a manner as one could wish.’” He then mentions some more recent researches, which, however, need not be introduced here.

tinacity of Nourse (who formed a higher opinion of him than was at alljust), rather than by any intrinsic merits of their own.

Of Emerson's personal character this is not the place to say much; and indeed it would be unnecessary to do more than refer to Hutton's Dictionary (*in loco*) for a description of his eccentricities, were it not for a *non-sequitur* that has been drawn from his and some similar cases. It surely does not follow that because Emerson and some others habitually indulged in a rough discourtesy of bearing towards others, that it arose from the nature of their studies, or inevitably followed from the tone of feeling generated by mathematics. Why not, then, charge medical studies with the same tendency on the ground of an Abernethy, or the legal on the ground of a Thurlow, belonging to those professions? The inference is indeed absurd enough; but many a time in my life have I heard it made.

However, I have to beg, once for all, that if the non-academic body of mathematicians are decreed to have a head of their school, they may at least be allowed their own choice. That election would fall, without a dissentient voice, on Thomas Simpson. Amongst themselves, he, and not Emerson, virtually fills the post; and they cannot but feel aggrieved by hearing a man whose character they do not respect and whose works they seldom open, thus held up as the prototype of themselves.

This series of notices of the Nourse papers must necessarily be incomplete without some account of the man who was the real focus of the mathematical literature of his time—now verging upon a century ago. I regret that few materials of a positive kind have fallen in my way from which I can satisfy so reasonable a desire on the part of my readers. Most of the letters are written in more or less of that dry, business-style, that only brings in an interesting incident now and then, and always casually. One series of letters, however, of a more familiar and intimate character, from John Robertson of Portsmouth (author of the *Treatise on Navigation*, and other works, and subsequently "Clerk" of the Royal Society), throw some light on Nourse's personal character. A few scraps, too, of Nourse's own geometrical speculations betoken a mind of no ordinary powers, and a taste in science such as even few professional mathematicians have evinced.

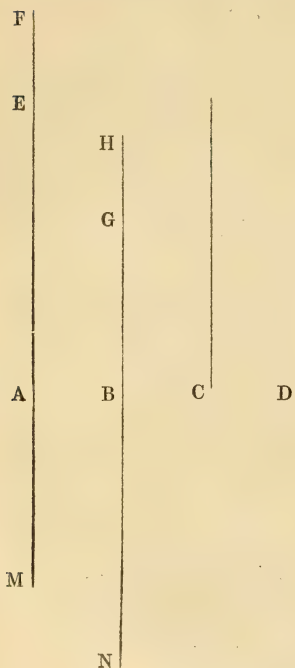
From the frequent jocular allusions of Robertson, it would appear that Nourse was a grave self-possessed person, who nevertheless enjoyed a pun or a good joke as well as his friend did. A quiet pipe with its adjuncts in the shop-parlour seems to have been his sum total of indulgence. He rarely quitted his

business; and when he did, it was only to go to Oxford where he had relatives—a brother, Sir Charles Nourse, and a sister, the wife of Dr. Hornsby, the Radcliffe astronomer. One of the letters seems to imply that he was a short and somewhat corpulent man, who always fancied himself to be “wasting away.” He was always alive to his business, and the number of works of which he was the publisher was unprecedented in his day; his undertakings appear to have been generally successful, and his dealings scrupulously honourable. He does not appear from any allusions in these letters to have been married: and he amassed considerable property, which was bequeathed to his brother and sister.

The mathematical character of Nourse is best shown by a specimen of his geometry. I therefore annex two: one a demonstration of the converse of Euc. v. 25, which he required as lemma for some emendations of a proposition on the conic sections in the works of Mylne and Simson; and the other a remarkably simple and elegant problem in *Angelis de inf. Parabolis*. These are given precisely as I find them in the MS., taking in the textual corrections made by himself.

“Lemma (Euclid v. 25 convers).

“Si quatuor magnitudines fuerint proportionales, et prima cum

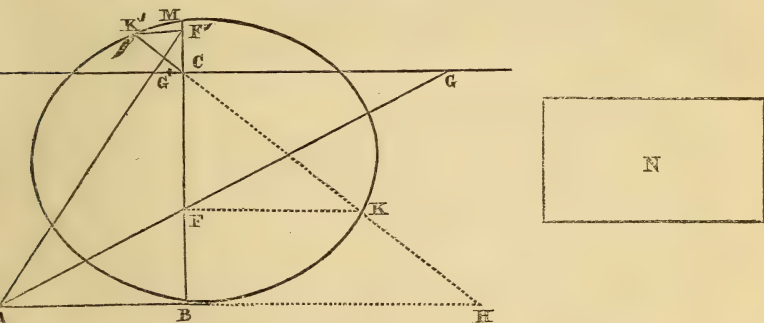


quartâ major fuerit secundâ cum tertiâ; erunt prima et quarta maxima et minima quatuor proportionalium.

“Sint enim proportionales AF. BH. C & D. Et quoniam prima cum quartâ major est secundâ cum tertiâ. non erit igitur prima AF tertia C. æqualis, sed vel major vel minor eâ. Sit primò major. fiatque ipsi C. æqualis AE. et ipsi D æqualis BG. et quoniam tota AF. est ad totam BH. ut ablata AE. ad ablatam BG ergo reliqua EF est ad reliquam GH. ut tota ad totam. Jam ipsi D. æqualis fiat AM. et ipsi C. æqualis BN. et erit MF. æqualis primæ cum quartâ. et NH æqualis secundæ cum tertiâ. Est itaque MF (ex hypothesi) major quam NH. Sed ME æqualis est NG. etenim ipsarum utraque equalis est C & D simul. Ergo, si ab æqualibus MF & NH. quarum MF major est, auferentur æquales ME & NG. residuæ erunt etiam inequales, nempe EF major erit quàm GH. Sed supra ostensum est EF. esse ut GH ut AF. ad BH. Ergo AF major

est quàm BH et C major quàm D. Sed ex hypothesi AF quàm C major, ergo BH quàm D & AF quàm D major. Ergo AF & D sunt maxima et minima quatuor proportionalium. Similiter ostendemas, si ponatur prima minor quàm tertiâ. Nam invertendo ordinem proportionalium ut quarta vocetur prima & tertia vocetur 2^{da}. et sic deinceps. eadem erit demonstratio. (nullo verbo mutato).

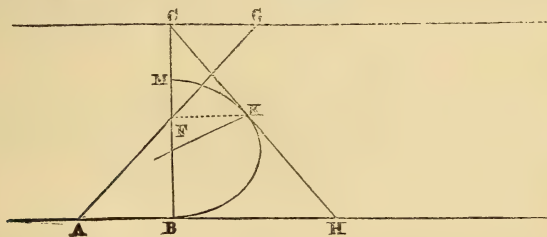
“ Prop 29, ad 36. Angelis De infinit. Parebolis p. 25 &c. ad 108.



“ Datis parallelis AB . CG, quas secit BC ad rectos angulos. data etiam puncto A : Ducere AG ita ut aggregatum triangulorum AFB. CFG equali sit spatío dato N.

“ Fiat AB in BM equali duplo spatío N. Quoniam igitur duplum N (vel AB in BM) equatur rectangulis $AB \times BF$ et $FC \times CG$ simul ; erit AB in FM equali rectangulo $FC \times CG$. unde erit AB ad CG, hoc est, BF ad FG, ut FC ad FM. Patet igitur solutio, ut sequitur.

“ Fiat enim BH equalis BC. et ducta circa diametrum BM. circulo MKB, qui secet CH in K. et ductâ KF parallelâ ipsi BA, quæ secet BC in F. Ducatur AG per F. dico factum. Patel ex analysi.



“ Insuper in hac analysi patet, aggregatum triangulorum AFB . CFG tum fore minimum, quando linea BM minima est omnium quæ conditionibus in analysi positis satisfaciant. Hoc autem evenit in illo casu ubi BM adeo parva vit, ut circulus hac diametro descriptus minimo secet lineam CH, sed tantum in uno puncto tangat. Fiat igitur HK (vel fiat $BF = \frac{1}{2}CH$) equalis HB et ducatur KF parallela AB. et ducta AF. triangua AFB . CFG simul sumpta minima facient spatium quod abscindi possit à quavis lineâ per punctum A ducta.

“ Quod ci spatium ad construendum propositum minus sit rect-

angulo ex AB in lineam datum CK (vel $\frac{1}{2}$ BM) problema construi nequit."

Though the determinations are very neatly given here, the circumstance of the double solution has escaped his notice, viz. the point of intersection of CH with the remaining semi-circle, as represented by the accented letters, which I have put in for the purpose of showing it. It will be worth the while of the younger geometrical reader to examine this case, and discover whether this second solution be that of the proposed problem or of a collateral one. It involves no material difficulty.

Shooter's Hill, Aug. 15, 1850.

XXV. *An Instantaneous Demonstration of Pascal's Theorem by the method of Indeterminate Coordinates.* By J. J. SYLVESTER, M.A., F.R.S.*

THE new analytical geometry consists essentially of two parts—the one determinate, the other indeterminate.

The determinate analysis comprehends that class of questions in which it is necessary to assume *independent* linear coordinates, or else to take cognizance of the equations by which they are connected if they are not independent. The indeterminate analysis assumes at will any number of coordinates, and leaves the relations which connect them more or less indefinite, and reasons chiefly through the medium of the general properties of algebraic forms, and their correspondencies with the objects of geometrical speculation. Pascal's theorem of the mystic hexagon, and the annexed demonstration of its fundamental property, belong to this branch of the subject, and afford an instructive and striking example of the application of the pure method of indeterminate coordinates.

Let x, y, z, t, u, v be the sides of a hexagon inscribed in the conic U. Let the hexagon be divided by a new line ϕ in any manner into two quadrilaterals, say $xyz\phi, tuv\phi$.

Then

$$ay\phi + bxz = U = au\phi + \beta tv;$$

$$\therefore (ay - au)\phi = \beta tv - bxz;$$

$\therefore ay - au$ and ϕ are the diagonals of the quadrilateral $txvz$.

By construction, ϕ is the diagonal joining x, v (*i. e.* the intersection of x and v) with z, t ; and thus we see that $ay - au$ is the line joining t, x with v, z ; but this line passes through y, u . Therefore $x, t; y, u; z, v$ lie in one and the same right line. Q. E. D.

26 Lincoln's-Inn-Fields,
August 1850.

* Communicated by the Author.

XXVI. *On a new Class of Theorems in elimination between Quadratic Functions.* By J. J. SYLVESTER, M.A., F.R.S.*

IN a forthcoming memoir on determinants and quadratic functions, I have demonstrated the following remarkable theorem as a particular case of one much more general, also there given and demonstrated.

Let U and V be respectively quadratic functions of the same $2n$ letters, and let it be supposed possible to institute n such linear equations between these letters as shall make U and V both simultaneously become identically zero†. Then the determinant of $\lambda U + \mu V$, which is of course a function of λ and μ of the $2n$ th degree, will become the *square* of a function of λ and μ of the n th degree; and conversely, if this determinant be a perfect square, U and V may be made to vanish simultaneously by the institution of n linear equations between the $2n$ letters‡.

Let now P and Q be respectively quadratic functions of three letters only, say x, y, z ; and let

$$U = P + (lx + my + nz)t$$

$$V = Q + K(lx + my + nz)t.$$

The determinant of $\lambda U + \mu V$ in respect to x, y, z, t is easily seen to be $(\lambda + k\mu)^2 \times$ the determinant of

$$\lambda P + \mu Q + (lx + my + nz)t$$

in respect to x, y, z, t . Hence if we call

$$\lambda P + \mu Q + (lx + my + nz)t = W,$$

and make $\begin{vmatrix} & \\ & \\ & \\ & \end{vmatrix}_{xyzt} \cdot W$ a squared function of λ, μ or which is the same thing, if

$$\begin{vmatrix} & \\ & \\ & \\ & \end{vmatrix}_{\lambda\mu} \cdot \begin{vmatrix} & \\ & \\ & \\ & \end{vmatrix}_{xyzt} \{W\} = 0,$$

U and V will vanish simultaneously when two linear relations are instituted between the quantities (all or some of them) x, y, z, t .

In order that this may be the case, it will be seen to be sufficient that

$$P=0 \quad Q=0 \quad (lx + my + nz)=0$$

* Communicated by the Author.

† In the more general theorem above alluded to, the number of letters is any number m , the number of linear equations being any number not exceeding $\frac{m}{2}$.

‡ When $n=1$, we obtain a theorem of elimination between two quadratics, which has been already given by Professor Boole.

shall coexist; for then two equations between x, y, z , of which $lx + my + nz = 0$ will be one, will suffice to make U and V each identically zero. Hence we have the following theorem:

$$\begin{vmatrix} \square \\ \lambda\mu \end{vmatrix} \begin{vmatrix} \square \\ xyz t \end{vmatrix} \{ \lambda U + \mu V + (lx + my + nz)t \}$$

is a factor of the resultant of

$$P = 0 \quad Q = 0 \quad lx + my + nz = 0.$$

A comparison of the orders of the resultant and the determinant shows that they must be identical, *à-ci-près*, of a numerical factor, which, if the resultant be taken in its *general* lowest terms, may no doubt be easily shown to be unity.

As an illustration of our theorem, let

$$P = xy + yz + zx$$

$$Q = cxy + ayz + bzx.$$

Then

$$\begin{vmatrix} \square \\ xyz t \end{vmatrix} \{ \lambda P + \mu Q + (lx + my + nz)t \}$$

$$= \begin{vmatrix} 0 & \lambda + c\mu & \lambda + b\mu & l \\ \lambda + c\mu & 0 & \lambda + a\mu & m \\ \lambda + b\mu & \lambda + a\mu & 0 & n \\ l & m & n & 0 \end{vmatrix}$$

$$= n^2(\lambda + c\mu)^2 + m^2(\lambda + b\mu)^2 + l^2(\lambda + a\mu)^2$$

$$- 2lm(\lambda + b\mu)(\lambda + a\mu) - 2mn(\lambda + c\mu)(\lambda + b\mu)$$

$$- 2nl(\lambda + a\mu)(\lambda + c\mu)$$

$$= \lambda^2 \{ n^2 + m^2 + l^2 - 2lm - 2mn - 2nl \}$$

$$+ 2\lambda\mu \{ cn^2 + bm^2 + al^2 - lm(a + b) - mn(b + c) - nl(c + a) \}$$

$$+ \mu^2 \{ c^2n^2 + b^2m^2 + a^2l^2 - 2ablm - 2bcmn - 2canl \}.$$

And we thus obtain, finally,

$$\begin{vmatrix} \square \\ \lambda\mu \end{vmatrix} \begin{vmatrix} \square \\ xyz t \end{vmatrix} \{ \lambda P + \mu Q + (lx + my + nz)t \}$$

$$= (n^2 + m^2 + l^2 - 2lm - 2mn - 2nl)$$

$$\times (c^2n^2 + b^2m^2 + a^2l^2 - 2ablm - 2bcmn - 2canl)$$

$$- \{ (cn^2 + bm^2 + al^2 - lm(a + b) - mn(b + c) - nl(c + a))^2 \}$$

$$= -4lmn \{ (a - b)(a - c)l + (b - a)(b - c)m + (c - a)(c - b) \}.$$

Now to obtain the resultant of

$$xy + yz + zx = 0$$

$$cxy + ayz + bxy = 0$$

$$lx + my + nz = 0,$$

we need only find the four systems in their lowest terms of $x:y:z$, which satisfy the first two equations, and multiply the four linear functions obtained by substituting these values of x, y, z in the fourth: the product will contain the resultant of the system affected with some numerical factor. In the present case, the four systems of x, y, z are

$$x=0 \quad y=0 \quad z=1$$

$$y=0 \quad z=0 \quad x=1$$

$$z=0 \quad x=0 \quad y=1$$

$$x=(a-b)(a-c) \quad y=(b-a)(b-c) \quad z=(c-a)(c-b),$$

and accordingly the product of

$$lx_1 + my_1 + nz_1$$

$$lx_2 + my_2 + nz_2$$

$$lx_3 + my_3 + nz_3$$

$$lx_4 + my_4 + nz_4$$

becomes

$$lmn\{\overline{a-b} \overline{a-c} l + \overline{b-a} \overline{b-c} m + \overline{c-a} \overline{c-b} . n\},$$

agreeing with the result obtained by my theorem,—a *special* numerical factor 4, arising from the peculiar form of the equations, having disappeared from the resultant.

A geometrical demonstration may be given of the theorem which is instructive in itself, and will suggest a remarkable extension of it to functions containing more than three letters,

$$\boxed{}_{xyzt} \{ \lambda U + \mu V + (lx + my + nz)t \} = 0,$$

which is a quadratic equation in $\lambda : \mu$, may easily be shown to imply that the conic $\lambda U + \mu V$ is touched by the straight line

$$lx + mz + nz = 0.$$

And we thus see that in general two conics,

$$\lambda U + \mu V = 0,$$

passing through the intersections of two given conics,

$$U=0 \quad V=0,$$

may be drawn to meet a given line. If, however, the given line passes through any of the four points of intersection, in such case only one conic can be drawn to touch it; accordingly

$$\boxed{} \boxed{} \{ \lambda U + \mu V + (lx + my + nz)t \}$$

must be zero when l, m, n are so taken as to satisfy this condition, *i. e.* if

$$lx_1 + my_1 + nz_1 = 0,$$

or

$$lx_2 + my_2 + nz_2 = 0,$$

or

$$lx_3 + my_3 + nz_3 = 0,$$

or

$$lx_4 + my_4 + nz_4 = 0,$$

whence the theorem.

Now suppose U and V to be each functions of four letters, x, y, z, t ; when

$$\left[\begin{array}{c} \square \\ xyztu \end{array} \right] \{ \lambda U + \mu V + (lx + my + nz + pt)u \} = 0,$$

the conoid $\lambda U + \mu V$ touches the plane

$$lx + my + nz + pt = 0;$$

and $\left[\begin{array}{c} \square \\ xyztu \end{array} \right] = 0$ being a cubic equation, in general three such conoids can be drawn.

Considerations of analogy make it obvious to the intuition, that in the particular case of two of these becoming coincident, the given plane $lx + my + nz + pt$ must be a tangent plane to those two coincident conoids at one of the points where it meets the intersections of $U=0$ $V=0$; *i. e.*

$$lx + my + nz + pt = 0$$

will pass through a tangent line to, or in other words, may be termed a tangent plane to the intersections. Hence the following analytical theorem, derived from supposing q, r, s, t to be proportional to the areas of the triangular faces of the pyramid cut out of space by the four coordinate planes to which x, y, z, t refer. As these planes are left indefinite, q, r, s, t are perfectly arbitrary.

Theorem.—The resultant of

$$\begin{array}{ll} 1. & U=0 \\ 2. & V=0 \end{array} \left. \vphantom{\begin{array}{l} 1. \\ 2. \end{array}} \right\} \begin{array}{l} \text{where } U \text{ and } V \text{ are functions of} \\ x, y, z, t. \end{array}$$

$$3. \quad lx + my + nz + pt = 0$$

$$4. \quad \left\{ \begin{array}{cccc} \frac{dU}{dx} & \frac{dU}{dy} & \frac{dU}{dz} & \frac{dU}{dt} \\ \frac{dV}{dx} & \frac{dV}{dy} & \frac{dV}{dz} & \frac{dV}{dt} \\ l & m & n & p \\ q & r & s & t \end{array} \right\} = 0;$$

which system, it will be observed, consists of three quadratic

functions, and one linear function of x, y, z, t , contains the factor

$$\begin{vmatrix} \square \\ \lambda\mu \end{vmatrix} \begin{vmatrix} \square \\ xyz t \end{vmatrix} \{ \lambda U + \mu V + (lx + my + nz + pt)u \}.$$

This last quantity is of the 4×3 th, *i. e.* the 12th order in respect of the coefficients in U and V combined; of the 4×2 th, *i. e.* the 8th order in respect of l, m, n, p ; and of the zero order in respect of q, r, s, t .

The resultant which contains it is of the $(4 + 4 + 2 \cdot 4)$ th, *i. e.* 16th order in respect to the coefficients in U and V ; of the $(4 + 8)$ th, *i. e.* the 12th, in respect of l, m, n, p ; and of the 4th in respect of q, r, s, t . Hence the special (and, as far as the geometry of the question is concerned, the unnecessary, I may not say extraneous or irrelevant) factor which enters into the resultant is of the 4th order in respect to the combined coefficients of U and V^* ; and of the same order in respect to l, m, n, p , and in respect to q, r, s, t .

I have not yet succeeded in divining its general value.

In the very particular example, of the system,

$$\begin{aligned} \alpha x^2 + \beta y^2 &= 0 \\ cz^2 + dt^2 &= 0 \\ lx + my + nz + pt &= 0 \\ \left. \begin{array}{cccc} \alpha x & \beta y & 0 & 0 \\ 0 & 0 & cz & dt \\ l & m & n & p \\ q & 0 & 0 & 0 \end{array} \right\} &= 0, \end{aligned}$$

I find that the double determinant is

$$c^2 d^2 \alpha^2 \beta^2 (cp^2 + dn^2)^2 (m^2 \alpha + l^2 \beta)^2,$$

and the resultant is

$$q^4 c^2 d^2 \alpha^2 \beta^4 (cp^2 + dn^2)^4 \cdot (m^2 \alpha + l^2 \beta)^2,$$

giving as the special factor

$$q^4 \cdot \beta^2 (cp^2 + dn^2)^2.$$

I believe that the theorem which I have here given for determining the condition that $lx + my + nz + pt$ shall be a tangent plane to the intersection of two conoids U and V , viz. that the determinant of

$$\lambda U + V + (lx + my + nz + pt)u$$

shall have two equal roots, is altogether novel.

* And consequently of the second in respect to the separate coefficients of each.

What is the meaning of all three roots of this determinant becoming equal, *i. e.* of only one conoid being capable of being drawn through the intersection of U and V to touch the plane

$$lx + my + nz + pt?$$

Evidently (*ex vi analogiæ*) that this plane shall pass through three consecutive points of the curve of intersection, *i. e.* that it shall be the osculating plane to the curve.

If we return to the intersection of two co-planar conics, and if we suppose a line to be drawn through two of the points of intersection, the conics capable of being drawn through the four points of intersection to touch the line, besides becoming coincident, evidently degenerate each into a pair of right lines. It would seem, therefore, by analogy, that if a plane be drawn including any two tangent lines to the curve of intersection of two surfaces of the second degree, this should be touched by two coincident cones drawn through the curve of intersection, and consequently every such double tangent plane to the intersection of two conoids (and it is evident that one or more of these can be taken at every point of the curve) must pass through one of the vertices of the four cones in which the intersection may always be considered to lie; and it would appear from this, that in general four double tangent planes admit of being drawn to the curve, which is the intersection of two conoids, at each point thereof. At particular points a tangent plane may be drawn passing through more than one of the vertices, and then of course the number of double tangent planes that can be drawn will be lessened. These results, indicated by analogy, become immediately apparent on considering the curve in question as traced upon any one of the four containing cones. For the plane drawn through a tangent at any point, and the vertex of the cone being a tangent plane to the cone, must evidently touch the curve again where it meets it. We thus have an additional confirmation of the analogy between a point of intersection of two curves and the tangent at any point of the intersection of two surfaces.

I might extend the analytical theorems which have been established for functions of three and four to functions of a greater number of variables; but enough has been done to point out the path to a new and interesting class of theorems at once in elimination and in geometry, which is all that I have at present leisure or the disposition to undertake.

26 Lincoln's-Inn-Fields,
August 13, 1850.

XXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 68.]

March 7, “**O**N the application of Carbon deposited in Gas Retorts as the negative plate in the Nitric Acid Voltaic Battery.” By Christopher Leefe Dresser, Esq. Communicated by Thomas Bell, Esq., Sec. R.S. &c.

In the retorts used for the destructive distillation of coal to obtain the carburetted hydrogen gas for the purposes of illumination, after a certain time a deposition of carbonaceous matter takes place, and which at length accumulates to such an extent as to fill up a portion of the retort with solid substance, and to line the whole with a coating varying from the thickness of paper to several inches.

After describing several forms in which this substance occurs, and which vary considerably both in density and hardness, the author states that he found one of great hardness, very little, if at all, porous, and of a stony fracture, to be best adapted for the negative conductor of his nitric acid battery. The most convenient form for the negative conductor is the prismatic, $1\frac{1}{8}$ inch square on the side and about 7 inches long, which is immersed 4 inches in the acid, and used with round porous cells, the zinc cylinder being 3 inches in diameter and $4\frac{1}{2}$ inches high.

The carbon is cut into thin plates or prisms by the machine of the marble cutter, at a cost of about $1\frac{1}{4}d.$ each. The prisms may be easily obtained 12, 14, or 18 inches long.

The only precautions necessary in using this form of carbon, are, after using the plates to immerse them for a few moments in boiling water, to take off the adhering acid, and then to dry them before a fire or in a stove.

Having used the same plates and prisms for months, the author detected no deterioration of their conducting power, nor any decomposition or alteration. The connexion was made by soldering a strip of sheet copper to the zinc, and pressing this strongly against the carbon with a clamp.

Comparing these plates with plates of platinum, the author could detect little difference in action, but the carbon appeared rather superior. He states that his battery of 100 plates cost under £4, whilst one of platinum of equal power would have cost £60 or £70. From the cheapness and durability of this substance, he considers that it will make a valuable addition to our voltaic apparatus.

A paper was also in part read, entitled “Experimental Researches in Electricity.” Twenty-third Series. § 29. On the Polar or other condition of Diamagnetic Bodies. By Michael Faraday, Esq., F.R.S. &c.

March 14.—The reading of Dr. Faraday’s paper, entitled “Experimental Researches in Electricity. Twenty-third Series. § 29. On the Polar or other condition of Diamagnetic Bodies:” was resumed and concluded. (See p. 88 of present volume).

March 21.—The following letter from Mr. Addington to the Secretary was read.

Foreign Office, March 20th, 1850.

SIR,—I am directed by Viscount Palmerston to send to you, for the information of the President and Council of the Royal Society, an extract of a letter which his Lordship has received from Mr. James Richardson, stating that in the month of November last, a fall of aërolites had taken place on the coast of Barbary attended with a brilliant stream of light, which extended from Tunis to Tripoli, some of the stones falling in the latter city.

I am, Sir,

Your most obedient, humble Servant,

H. W. ADDINGTON.

The Secretary to the Royal Society.

*“Extract of a letter from Mr. Richardson, dated off Jerbah,
25th January 1850.*

“I will trouble your Lordship by the mention of the astronomic phenomenon which terrified or arrested the attention of the inhabitants of the whole of this coast some two months ago. This was the fall of a shower of aërolites, with a brilliant stream of light accompanying them, and which extended from Tunis to Tripoli, some of the stones falling in the latter city.

“The alarm was very great in Tunis, and several Jews and Moors instinctively fled to the British Consulate, as the common refuge from every kind of evil and danger.

“The fall of these aërolites was followed by the severest or coldest winter which the inhabitants of Tunis and Tripoli have experienced for many years.”

The reading of a paper, entitled “Discussion of Meteorological Observations taken in India at various heights.” By Lieut.-Colonel Sykes, F.R.S. &c., was commenced, but was not concluded.

April 11.—Lieut.-Colonel Sykes’s paper, entitled “Discussion of Meteorological Observations in India,” was resumed and concluded.

The author adverts to a former paper “On the Meteorology of the Deccan,” published in the Philosophical Transactions for 1835, and after referring to the conclusions at which he arrived in that communication, states that, in the discussion of the meteorological observations which form the subject of the present paper, and which were made over a very extended area, at different heights, some being hourly and running through several years at the same station, it is very satisfactory to find that they fully establish the accuracy of the former deductions. He remarks that, as some of the observations now discussed were hourly records continued through considerable periods of time, an opportunity has been afforded of investigating abnormal conditions, which the former limited number of diurnal observations did not permit; and gives the following review of what appears to be normal and abnormal conditions.

The annual and daily range of the barometer diminishes from the sea-level up to the greatest height observed, 8640 feet at Dodabetta,

from a mean annual and mean daily range at Madras of 0·735 and 0·122 respectively to 0·410 and 0·060 at Dodabetta;—the annual range would appear to increase, about and beyond the northern tropic, as the annual range at Calcutta (not by hourly observations) is 0·911; but the diurnal range is somewhat less (0·115) than at Madras. At no one of the places of observation, even taking the maximum pressure of one year with the minimum pressure of another year, does there appear to have been a range of pressure equivalent to an inch of mercury; nevertheless in the Cyclones, or rotatory storms, there occurs at times a range of pressure of nearly two inches of mercury within forty-eight hours; but it is shown from a comparison of the simultaneous records on board ship, where these great depressions were noted, with the records at the observatories on shore, that the great depressions occurred within very limited areas.

The author had formerly shown that the times or turning-points of ebb and flow (if the term be permitted) of the ærial ocean, were occasionally retarded or accelerated, although the means fixed the turning-points within certain limit hours; but he was not then aware that in the ebb or flow of the four daily tides, they ever retrograded or halted in their onward or retiring course. The hourly observations now show that abnormal conditions are of no infrequent occurrence,—that the tides at times flow or ebb for four, five, six or even seven and eight hours (one instance at Aden of nine hours),—that frequent instances occur of retrograde movements for short periods of time, as if the tide had met with a check and been turned back; and at the turning-points there are numerous instances of the atmosphere being stationary for a couple of hours.

The maximum pressure of the atmosphere is in the coldest months, December or January, but the minimum pressure is not in the hottest months, but in June or July. The barometric readings, when protracted, show a gradual curve from December or January descending to June or July, and then ascending again to December or January, there being an occasional interruption in October or November. As the curves at Madras, Bombay and Calcutta, correspond, and as Madras has *no* south-west monsoon, while Bombay has a south-west monsoon, and is destitute of the north-east monsoon of Madras, it would appear that the general movements of the atmosphere are little influenced by any conditions of its lower strata; but the curve of pressure would seem to have some relation to the sun's place in the ecliptic.

The normal conditions of daily temperature are, that it is coldest in India at sunrise, and hottest between the hours of 1 and 3 P.M.; but the tables show many aberrations from this rule. The regular increment or decrement of mean monthly heat from the maximum or minimum period is somewhat remarkable, as the curve is independent of the south-west monsoon at Bombay and the north-east monsoon at Madras; and the passage of the sun twice over both places does not derange the curve. The anomalies of the annual mean temperature of Madras, Bombay, Calcutta and Aden, not diminishing with the increase in the latitude of the respective

places, are pointed out, and numerous instances are given of the very great power of the slanting rays of the sun beyond the tropic. As is the case with the barometric, so do the heat tables indicate that the annual and daily ranges of the thermometer diminish with the elevation of the place of observation above the sea-level, the elevated table-land of the Deccan however being an exception to this rule. At Mahabuleshwur, at 4500 feet, the temperature of the air was never below 45° with a maximum and minimum thermometer; and at Dodabetta the temperature of the air was never below $38^{\circ}5$, nevertheless at both places ice and hoar-frost were frequently found on the ground at sunrise, resulting from the separate or conjoined effects of radiation and evaporation.

After stating the want of confidence he has in observations of the wet-bulb thermometer as a means of determining the dew-point, and that he greatly prefers Daniell's hygrometer for this purpose, the author observes that he will not venture to say more with respect to normal conditions of moisture in India than that the air of the sea coast has always a much greater fraction of saturation than the lands of the interior; and that the elevated plateau of the Deccan is periodically subject to very high degrees of dryness.

Some very unexpected phenomena with reference to the distribution of rain are pointed out. It is found both on the sea coasts and on the table-lands of the Deccan, that within very limited areas, the differences in the fall of rain may be very great. With nine rain-gauges employed in the small island of Bombay in the months of June and July, in the monsoon of 1849, the quantity collected in the different gauges ranged in July from 46 inches to 102 inches, and in June from 19 inches to 46 inches. At Sattarah, in the Deccan, with three rain-gauges within the distance of a mile, they differed in their contents several inches from each other; and at Mahabuleshwur and Paunchgunny, nearly on the same level, the latter place being only eleven miles to the eastward of the former, the annual fall of rain was 254 inches and 50 inches respectively! The normal conditions are, that there is a much greater fall of rain on the sea coasts than on the table-lands of the Deccan, but that the Ghats intervening between the coasts and the table-lands have three times the amount of the fall on the coasts, and from ten to fifteen times the amount of the fall on the table-lands of the interior; the paucity of the fall of rain at Cape Comorin and in the mouths of the Indus would also appear to be normal conditions.

The tables must be referred to for the winds; the normal states are those of the south-west and north-east monsoons, and the influence of the latter is periodically felt at the height of 8640 feet at Dodabetta, which height would appear just at the upper surface of the stratum of air constituting the south-west monsoon; but hourly observations for lengthened periods are necessary at Dodabetta, to determine what really are the periodical winds at that height. From the points other than those between south and west, and north and east, there is also at the several stations a certain amount of periodicity in the winds, the winds that are common to different stations having only a slant

more or less at the different stations ; for instance, the south-west and north-west winds of Bombay blowing in the summer months in Calcutta incline rather to be south and north winds, than south-west and north-west winds ; but the author observes that to be enabled to speak with any precision upon this branch of the meteorology of India, and indeed upon most other branches with a comprehensive and philosophical object, hourly observations are necessary,—simultaneously taken with previously compared instruments by zealous observers ; and having the records in a form common to all the observers, so as to admit of rigid comparisons :—when this is done, not only in India but in Europe, meteorologists will be in a better condition to generalize and propound normal conditions, than the state of our knowledge at present would justify.

The author states that he is indebted to that very able and zealous meteorologist, Dr. Buist of Bombay, for the protracted curves of pressure of the barometer appended to his paper.

A paper was also read, entitled “On the Structure and Use of the Ligamentum rotundum Uteri, with some observations upon the change which takes place in the structure of the Uterus during Utero-gestation.” By G. Rainey, Esq., M.R.C.S.E., Demonstrator of Anatomy, St. Thomas’s Hospital. Communicated by Joseph H. Green, Esq., F.R.S.

The author first refers to the discovery of the difference which exists between the two classes of muscles ; the voluntary, or those with striped fibres, and the involuntary, or those with unstriped fibres. He then notices that the opinion which is entertained respecting the round ligaments being composed of the unstriped variety of muscular fibre is incorrect, these organs consisting chiefly of the striped muscular fibre.

In support of the accuracy of this assertion, the author alleges the following facts :—

First, that the round ligament arises by three tendinous and fleshy fasciculi ; one, from the tendon of the internal oblique, near the symphysis pubis, a middle one from the superior column of the external abdominal ring, the third from the inferior column of the same : from these points the fibres pass backwards and outwards, and uniting form a rounded cord—the round ligament ; after which, traversing the broad ligament, they go to be inserted into the angle of the uterus.

The striped fibres are principally situated in its centre, and extend from its origin to within an inch or two of the fundus uteri ; as they approach which, the fibres gradually lose the striated character and degenerate into fasciculi of granular fibres of the same kind as those of the Dartos muscle ; both these fibres presenting similar microscopic characters when acted upon by glycerine.

The author then states that the round ligament does not pass through the external ring to be lost in the labia and mons veneris ; and argues from the fact of their consisting mainly of striped fibres, &c., that their use cannot be merely mechanical or subservient to the process of utero-gestation, and therefore he concludes that its

function must be connected in some way with the process of copulation.

He also adverts to the necessity of examining the round ligament by the microscope in glycerine in preference to any other fluid; as this substance renders the cellular tissue mixed with the fibres more transparent without diminishing the distinctness of their characteristic markings. The author next states his views on the changes which take place in the uterus during utero-gestation, and observes, first, that there is no similarity between the fibres of the round ligament and those of the unimpregnated uterus, the latter being made up of spindle-shaped nucleated fibres, contained in a matrix of exceedingly coherent granular matter; that these fibres are best examined in portions which have been broken up by needles, in preference to thin sections; and that this tissue is well seen in the larger mammals, as in the Cow, &c. In the impregnated uterus the fibres are found much increased in size and distinctness, but devoid of nuclei and comparatively loosely connected; and the enlargement of these fibres is of itself sufficient to account for the increased volume of the gravid uterus, without supposing that a set of muscular fibres are formed in it *de novo*.

Hence he reasons that the unimpregnated uterus consists probably of little more than an assemblage of embryonic nucleated fibres, inactive until the ovum is received into it, after which their development commences and continues simultaneously and progressively with that of the foetus; so that when this last has arrived at a state requiring to be expelled, the uterus has acquired its greatest expulsive power. Lastly, the author observes, since the fully-developed fibres cannot return to their former embryonic condition, they necessarily become absorbed, and a new set of embryonic fibres are formed for the next ovum, so that each foetus is furnished with its own set of expulsive fibres; which view is in perfect accordance with the statements of Drs. Sharpey and Weber, with regard to the *membrana decidua*.

April 18.—“On the Solution of Linear Differential Equations.” By the Rev. Brice Bronwin, M.A. Communicated by S. Hunter Christie, Esq., Sec. R.S.

The methods employed in this paper to effect the solution or reduction of linear differential equations consist of certain peculiar transformations, and each particular class of equations is transformed by a distinct process peculiarly its own. The reduction is effected by means of certain general theorems in the calculus of operations.

The terms which form the first member of the first class of equations are functions of the symbols ϖ and τ , the latter being a function of x , and the former a function of x and D , x being the independent variable. This member of the equations contains two arbitrary functions of ϖ , and may therefore be of any order whatever. It likewise contains two simple factors, such for example as ϖ and $\varpi + nk$, which factors are taken away by the transformation employed, and consequently the equation is reduced an order lower; it is therefore integrated when of the second order. There is a se-

ries of equations of this class, each essentially distinct from the rest, yet all reducible by a similar process.

These equations contain two arbitrary functions of x . The number therefore of particular practicable forms, which may be deduced from each, is very great, a circumstance which renders our chance of putting any proposed equation under one of these forms greater in the same proportion. On account of the very large number of particular integrable equations which each general example furnishes, selection would be very difficult, and all could not be given; the author has therefore refrained from giving any.

The second class of equations may be deduced from the first by the interchange of the symbols D and x , and changing τ into τ^{-1} . The second general theorem can be deduced from the first in like manner; and this class may be transformed and reduced by it in a manner exactly similar to that by which the former class is reduced by the first general theorem. The solution therefore of the one series may be deduced from that of the other by the interchange of symbols only. But in the second series the solutions obtained are not always practicable, that is to say, they cannot always be interpreted in finite terms. They have therefore been reduced by the introduction of new arbitrary functions of D , which render them practicable; this process however necessarily diminishes their generality.

When reduced to the ordinary form, these equations are somewhat complicated; but by giving suitable forms to the arbitrary functions of D which they contain, we may derive from them particular examples of a form as simple as we please, and by introducing as many arbitrary constants as possible, these examples may be made very general of the class to which they belong. In the integration of linear equations, the coefficients of which are integer functions of x , they may prove very useful.

Next, an equation, a particular case of which was treated by Mr. Boole in the Cambridge Mathematical Journal, is here integrated under its most general form. Instead of integer functions of x , the coefficients may be any functions whatever, consistent with the condition of integrability, which is ascertained, and the formulæ of reduction assumed by Mr. Boole are shown to be universally true. An additional function of the independent variable is also introduced into the operating symbol π . The equation therefore, independently of the condition of integrability, contains two arbitrary functions of x , and consequently gives rise to a considerable number of particular integrable examples.

Here also the interchange of the symbols D and x is made, both in the equation to be integrated and in the general symbolical theorem by which it is reduced, and the same reduction to practicable forms as before is likewise made.

The next class of equations results from the generalization of another equation integrated by Mr. Boole in the Cambridge Mathematical Journal. Here the symbol D of Mr. Boole is replaced by the general symbol ϖ , and moreover the first member of each equation contains two arbitrary functions of ϖ ; and by means of another

extension, this example gives rise to a whole series of equations constituting a class. The reduction is effected partly by the first general theorem in the calculus of operations, and partly by other means. It must be observed that each of the classes is totally distinct from the others, and its mode of treatment also distinct; also each of the general examples in the series contains two arbitrary functions of the independent variable, and will therefore give the solutions of a large number of particular equations, but for the reason before stated particular examples are not given.

Here likewise, by the interchange of the symbols D and x , another series of equations with their solutions or reductions is obtained, and also another general theorem by which they may be transformed and reduced. But the solutions of the examples of the one series may be deduced from those of the other by the interchange of symbols. It is not a little remarkable that this interchange of symbols in all these cases should be found possible, it will however be found possible in another case to be hereafter described.

The last class of equations discussed in this paper is transformed by means of a general theorem of a very different kind from any of those which have been employed in reducing and integrating any of the previous classes. By means of this transformation, the symbol ω , of which the first member of these equations is a function, is placed in a position to operate upon the whole of that member, a certain equation of condition among the coefficients being previously admitted. Hence by operating upon both members with the inverse of this symbol, the equation is once integrated, and, if it be of the second order only, completely solved.

Here too the interchange of symbols may be made both in the equation and its solution, and the solution so changed will be the solution of the equation changed in like manner. The general symbolical theorems, which here consist of a series of terms, may be derived the one from the other in the same way, and by changing the signs of the alternate terms.

Reductions of the arbitrary functions of D , similar to those before made, are made here also; and by particularizing some of the functions so reduced for the sake of simplification, several very singular resulting equations are obtained. If in these we assign to the remaining arbitrary functions, particular forms, and introduce as many arbitrary constants as we can, we may find particular examples which may be of great use in the integration of equations with coefficients containing only integer functions of x .

By a very obvious substitution an arbitrary function of x may be introduced into any of this kind of equations, and also another function of D , and the last often with great advantage.

"On the Oils produced by the Action of Sulphuric Acid upon various classes of Vegetables," by John Stenhouse, Esq., F.R.S.

Nearly thirty years ago Döbereiner observed, when preparing formic acid by distilling a mixture of starch, peroxide of manganese and sulphuric acid, that the liquid which passed into the receiver contained a small quantity of oil which rendered it turbid. To this oil Döbereiner gave the fanciful name of "artificial oil of ants," though

the very limited quantity in which he was able to procure it prevented him from determining almost any of its properties.

The author's attention was first directed to the subject in 1840, when he found that the oxide of manganese was unnecessary, and that the oil could be readily prepared by operating on most vegetable substances with either sulphuric or muriatic acid. The oil, on analysis, was found to have the formula $C_{15}H_6O_6$, and to contain oxygen and hydrogen in the proportions to form water, while all other oils and fats contain an excess of hydrogen.

The late Dr. Fownes took up the subject in 1845, and made the interesting discovery, that when the oil which he called furfurole is heated with ammonia, a crystalline amide is formed. When this amide is boiled with caustic lyes, it is changed into the crystallizable base furfurine. The paper then describes the best mode of preparing furfurole, and also the method of purifying it from an oil with which crude furfurole is always accompanied, and to which the name of meta-furfurole has been given. Meta-furfurole is the cause of the bright red coloration which impure furfurole instantly produces when it is treated with muriatic or sulphuric acids in the cold. This portion of the paper concludes with some new observations on furfurole, and an examination of some of the salts of furfurine.

It has been pretty satisfactorily ascertained that the constituent of plants which yields furfurole is the *matière incrustante* of M. Payen, viz. the matter with which the interior of the cells of plants is lined. This is an amorphous granular substance which has been gradually deposited from the sap in its passage through the tissue of the plant. It is most abundant in hard woods, such as oak and teak, especially in the oldest portions which lie nearest the heart of the tree. As the author of the paper was led to conjecture that the *matière incrustante* of the different great classes of vegetables would be found on examination analogous but not identical, he thought it likely that the oils derivable from them would also prove not identical with furfurole, though probably very analogous to it in their nature and properties. The algæ or sea-weeds, whose structure is very different from that of ordinary herbaceous plants, were employed to test the truth of this hypothesis. They yielded an aromatic oil to which the name of fucosole was given. Though essentially different from furfurole, it closely resembles that oil in its properties, being also isomeric with it. Fucosole forms a crystallizable amide with ammonia, called fucus amide, which, when it is boiled with alkaline lyes, is also converted into an organic base—fucusine, which is likewise isomeric with furfurine. Fucusine is a rather difficultly crystallizable base; but some of its salts, especially the nitrate, may be readily procured in large crystals. In solubility and crystalline form they differ from those of the corresponding base.

The paper contains an analysis of these salts.

The mosses and lichens were also found to yield fucosole. The ferns, on the other hand, yield a peculiar oil, which differs both from fucosole and furfurole, possessing properties intermediate between them.

The results of these experiments seem to indicate some curious

botanical relations, as it appears highly probable that the *matière incrustante* is the same in all phanerogamous plants, as they yield furfurole. On the other hand, the *matière incrustante* in the Algæ, mosses and lichens, as it yields fucusole and not furfurole, though the same in each of these classes, is evidently different from that of phanerogamous plants. The *matière incrustante* of ferns appears however to be dissimilar from either of the others, as it yields an analogous but peculiar oil.

April 25.—“On the means adopted in the British Colonial Magnetic Observatories for determining the absolute values, secular change, and annual variation of the Magnetic Force.” By Lieut.-Col. Edward Sabine, R.A., For. Sec. R.S.

The determination of the mean numerical values of the elements of terrestrial magnetism in direction and force at different points of the earth's surface (the force being expressed in absolute measure, intelligible consequently to future generations, however distant, and conveying to them a knowledge of the present magnetic state of the globe), and the determination of the nature and amount of the secular changes which the elements are at present undergoing, are, as the author states, the first steps in that great inductive inquiry by which it may be hoped that the inhabitants of the globe may at some date, perhaps not very distant, obtain a complete knowledge of the laws of the phenomena of terrestrial magnetism, and possibly gain an insight into the physical causes of one of the most remarkable forces by which our planet is affected.

After stating the inadequacy of the instruments originally proposed by the Royal Society, to the attainment of all the objects for which they had been designed, the author refers to the modifications which had been introduced, in the instruments and methods of observation for the determination of the absolute values, and the secular changes of the horizontal component of the magnetic force. He then gives the series of the results of the monthly observations at Toronto from January 1845 to April 1849 as relatively correct; and from this series, regarding each monthly determination as entitled to equal weight, and taking the arithmetical mean of all the values as the most probable mean value, obtains 3.53043 as the mean value of the horizontal force at Toronto, with a probable error of $\pm .00055$; and the probable error of $\pm .0040$ for each monthly determination.

This is on the most simple hypothesis, in which neither secular change nor annual variation is supposed to exist. The monthly results however distinctly indicate a secular change, and by means of them, on the hypothesis of a uniform secular change, the author deduces .0042 as the annual decrease of the horizontal force during the period comprehended by the observations, the value of the force on the 1st of March 1847, the mean epoch being 3.53043, with a probable error of $\pm .00025$.

For the purpose of deducing the values of the total magnetic force and its secular change from those of the horizontal force, it is necessary to know the magnetic inclination corresponding to the epoch and its secular change. From the observations of the inclination, $75^{\circ} 16' 09$ is deduced as the value of this element on the 1st of March

1847, with a secular increase of $0^{\circ}89$ annually; and $13^{\circ}8832$ as the value of the total force in absolute measure, at the same epoch. As the annual increase of $0^{\circ}89$ in the inclination will not account for an annual decrease of more than $\cdot0033$ in the horizontal force, there remains $\cdot0009$ as indicative of a small annual decrease in the total force during the period of the observations, and the author considers that the probabilities are in favour of such a decrease.

The general fact of an annual variation of the horizontal force at Toronto, the force being greater in the summer than in the winter months, is shown by three independent methods of experiment, viz. the observations from which the foregoing conclusions have been drawn, the regular observations with the bifilar magnetometer, and observations undertaken expressly with the view of ascertaining the fact. The author also infers the probable existence of an annual variation of the total force, the force being greatest in the winter months, or when the sun is in the southern signs, and least in the summer months, or when the sun is in the northern signs.

The results obtained from the observations at Hobarton are next briefly stated. The investigation, conducted in the same manner as at Toronto, shows at Hobarton a decrease of *south* inclination of $0^{\circ}89$ on the average of the months from April to August inclusive, that is, in the southern winter; and an increase of $0^{\circ}85$ from October to February inclusive, that is, in the southern summer.

The series of observations on the horizontal force shows an annual variation of the same character as respects the seasons, and almost identical in amount with that at Toronto. In the months from October to February inclusive, or in the summer months at Hobarton, the horizontal force is $\cdot0017$ greater on the average than its mean amount; and from April to August inclusive, or in the winter months at Hobarton, it is on the average $\cdot0013$ less than its mean amount.

The inferences drawn from these variations of the inclination and horizontal force, taken jointly as respects the total force at Hobarton, are that this force is subject to an annual variation, being higher than its mean amount from October to February, and lower than its mean amount from April to August.

It thus appears that in the months from October to February the magnetic needle more nearly approaches the vertical position, both at Toronto in the northern hemisphere, and at Hobarton in the southern; and that the total force is greatest at both stations from October to February, and least from April to August.

It is much to be desired, the author states, that so remarkable a result should receive a full confirmation, by the continuance of the observations at Toronto and Hobarton for such an additional period as may appear necessary for that purpose; and that the general conclusion, indicated by the observations at those stations, should be verified by similar investigations in other parts of the globe, especially at the observatories which now exist. He conceives that these facts indicate the existence of a general affection of the whole globe having an annual period, and would appear to conduct us naturally to the position of the earth in its orbit as the first step towards an

explanation of the periodic change. He further urges the importance of following up without delay, and in the most effective manner, a branch of the research which gives so fair a promise of establishing, upon the basis of competent experiment, a conclusion of so much theoretical moment.

In conclusion the author adverts briefly to considerations which may give a particular importance to accurate numerical values of the magnetic elements and their secular changes at Toronto, namely the proximity of that station to one of the two points on the northern hemisphere, which are the centres of the isodynamic loops, and are the points of the greatest intensity of the force (on the surface of the globe) of apparently two magnetic systems, distinguished from each other by the very remarkable difference in the rate of secular change to which the phenomena in each system appear to be subject.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 153.]

March 11, 1850.—On the Knowledge of Body and Space. By H. Wedgwood, M.A.

No part of the great metaphysical problem chalked out by Locke has been more assiduously laboured, and none has attained a less satisfactory solution, than that which relates to the origin of the idea of space and its subordinate conceptions, figure, position, magnitude.

It was seen that the exercise of the muscular frame must somehow be instrumental in making us acquainted with the material and extended world, but all hopes of a logical explanation of the process by which that effect is produced seemed cut off at the outset by a preliminary objection. The knowledge of motion, it was said, obviously involves the knowledge of the body moved. The consciousness of the motion of the hand therefore implies the conception of the hand itself, an object of certain shape and size. The attempt to account for the notions of shape and size from the motion of the hand was thus apparently stranded in a hopeless paralogism; and so insurmountable was the difficulty taken to be, that philosophers were driven to imagine a second source of elementary ideas, in addition to the simple apprehension of the thing conceived in actual existence, maintaining that space is known to us as the *condition* under which we perceive external things, or, as others express it, that the notion of space arises in the mind on the first apprehension of body by a principle of necessary judgement, which impresses upon us the conviction that all body is contained in space.

In the paper laid before the Society, an attempt is made to show the utter barrenness of this hypothesis of a necessary origin (as it is called) of the idea of space; and the main object of the paper is to rest the idea on a more solid foundation, by showing the adequacy of muscular exertion, in conjunction with the sense of touch, to furnish us with complete knowledge of the material and extended world by the ordinary way of actual experience.

There are two kinds of action; one *instinctive*, immediately in-

duced by the physical constitution of the agent independent of the understanding; the other *rational*, induced by the discernment of some object of desire in the end to be accomplished, and of course implying a previous conception of the action in question.

Familiar instances of instinctive action are then pointed out, from whence it would appear that the sensations of touch felt on contact of any part of the living frame with a foreign body operate as motives to instinctive exertion through the instrumentality of that part of the muscular frame on which the sensible impression is made, instinctively impelling the sentient being to muscular reaction against the material cause of the sensation, or leading him to shrink from it if the sensation is of a painful nature.

Attention is directed in particular to the action of an infant instinctively closing his hand upon a finger placed within his palm; and it is argued that the effect of such an action on his understanding will be the direct apprehension of *body*, a complex object consisting of *surface* (undeveloped as yet in form and magnitude) apprehensible by tactual sensation; and *substance*, revealed by resistance to muscular exertion, constituting a new kind of being essentially different from any of those discerned by means of the five senses.

The relation between body and space is illustrated by comparison with the case of light and darkness, the second of the two correlatives belonging in each case to Locke's class of *positive ideas from negative causes*. As he who has once apprehended light is subsequently enabled to look for that phænomenon in a direction from whence no rays actually penetrate the eye, so, it is argued, will he who has once made use of his hand in the apprehension of body be enabled to stretch out the same member in feeling for body when none is actually within reach; and as in the former case the failure of the effort to discover light results in the sensible impression of black or darkness, so in the latter case the effort unsuccessfully aimed at the apprehension of body will take effect on the intelligence in the direct cognition or actual experience of space, viz. of that particular portion of space through which the hand is moved in the unsuccessful search after body.

Thus the notion of space, like that of body, or of any sensible phænomenon, is traced to the actual experience of the thing itself in concrete existence. The subsequent enlargement of the idea, so as to comprehend the space occupied by the solid substance of bodies and that which stretches away to infinity in all directions around us, is duly accounted for on the same principle; and that impossibility of conceiving the destruction of any portion of space, on which so much stress has been laid as establishing the necessity of a deeper-seated origin than simple experience, is shown to be the natural consequence of the negative foundation of the idea as explained by the analogy of light and darkness.

May 13.—Results* connected with the theory of the singular solu-

* This communication is the abstract of a part of a paper not yet completed, and was forwarded to the Society for the purpose of ascertaining whether any examples could be produced destructive of the perfect generality of the results.

tion of a Differential Equation of the first order between two variables. By Professor De Morgan.

By a singular solution of a differential equation is here meant any solution which can be obtained by differentiation only, whether it be a case of the primitive by integration or not.

By a curve is meant all that is included under one equation, whether resolvable into what are commonly called complete curves or not. Thus, the equation

$$(x-y)(x^2+y^2-1)=0$$

belongs to a curve, having a rectilinear branch and a circular one.

By such a symbol as v_x is meant the partial differential coefficient $\frac{dv}{dx}$, when obtained from an equation in which v is explicitly expressed in terms of x and (it may be) other variables.

Let $\phi(x, y, c)=0$ be the complete primitive of the differential equation $y'=\chi(x, y)$.

$\phi(x, y, c)$ belongs to two distinct classes of curves:—

1. Continuous curves derived from such values of c , real or imaginary, as will enable $\phi=0$ to exist for points infinitely near to one another.

2. Systems of points, derived from

$$A(x, y, \alpha, \beta)=0, \quad B(x, y, \alpha, \beta)=0,$$

where

$$\phi(x, y, \alpha + \beta \sqrt{-1}) = A(x, y, \alpha, \beta) + B(x, y, \alpha, \beta) \cdot \sqrt{-1}.$$

When a curve is such that the points on one side of it are on curves of the first kind, and those on the other side are part of systems of the second kind, let that curve be called a *separator*; and the same when it separates points of both kinds from points which belong to one kind only.

No solution of the differential equation can be formed by combining all those systems of the second kind in which α and β are connected by a real relation.

The curve which has at every point of it, either

$$\frac{\phi_x}{\phi_c} = \infty, \quad \frac{\phi_y}{\phi_c} = \infty,$$

or

$$\frac{\phi_x}{\phi_c} = \infty, \quad \frac{\phi_y}{\phi_c} \text{ finite, } x = \text{const.},$$

or

$$\frac{\phi_y}{\phi_c} = \infty, \quad \frac{\phi_x}{\phi_c} \text{ finite, } y = \text{const.},$$

is a singular solution. And in the above are contained *all* the singular solutions.

Every branch of a singular solution is either—

A separator, only.

A curve, every point of which has a contact of the first order at least with some one real primitive, only. Or both. Or neither.

If the first or last, it is a case of the complete primitive. And

such cases may be introduced at pleasure into the singular solution, by writing the primitive in the form

$$\phi(x, y, fc) = 0.$$

A branch of a singular solution has at the utmost n contacts with each primitive which it touches (n being determined by the nature of the equation), and all of the first order, generally. Or, p_1 of the first order, p_2 of the second, &c., $p_1 + 2p_2 + 3p_3 + \dots$ being n or $n -$ (an even number); or some of these cases for some primitives, others for others, including the possibility of some cases giving none at all, when n is even.

The branches of the singular solution which have contact with ordinary primitives (whether themselves ordinary primitives or not) to the exclusion of the branches which are only separators, may be determined from the differential equation by the following test.

Let $y' = \chi(x, y)$ be the differential equation; whence

$$y'' = \chi_x + \chi_y \cdot \chi.$$

Find the curves which satisfy either of the following sets of conditions:—

$$\chi_x = \infty \quad \chi_y = \infty \quad y'' \text{ finite,}$$

or

$$\chi_x = \infty \quad x = \text{const.} \quad y'' \text{ finite,}$$

or

$$\chi_y = \infty \quad y = \text{const.} \quad y'' \text{ finite.}$$

Every such curve *does* satisfy the differential equation, and is a *singular solution* having contact with some one primitive at every point.

And other such singular solutions there are none except those designated by $x = \infty$, or $y = \infty$, or both.

But if

$$\chi_x = \infty \text{ and } \chi_y = \infty \quad y'' = \infty,$$

or

$$\chi_x = \infty \quad x = \text{const.} \quad y'' = \infty,$$

or

$$\chi_y = \infty \quad y = \text{const.} \quad y'' = \infty,$$

then the differential equation may or may not be satisfied; but the curve passes through the singular points of the primitives, with or without contact, according as the differential equation is or is not satisfied. An evolute is such a pseudo-singular solution to all the involutes, passing through their cusps.

XXVIII. *Intelligence and Miscellaneous Articles.*

ON THE COMPOUNDS OF IODINE AND PHOSPHORUS.

M. B. CORENWINDER remarks that the combinations of iodine and phosphorus have been hitherto unknown, means not having been found for obtaining them in a definite state and in the crystalline form. A method however exists of preparing these bodies crystallized. It consists in successively dissolving phosphorus and iodine in sulphuret of carbon and cooling the solution. Crystals of iodide of phosphorus are soon deposited, the composition of which depends upon the quantities employed.

In operating on two equivalents of iodine and one of phosphorus, M. Corenwinder obtained prismatic crystals of large dimensions, of an orange colour, and which by analysis gave the composition I^2Ph . This is the protiodide of phosphorus. It melts at $230^\circ F.$, alters by exposure to the air, and volatilizes at a higher temperature. It may be advantageously employed for preparing hydriodic acid.

By taking 3 equivalents of iodine and 1 of phosphorus and concentrating the solution, irregular crystals of a deep reddish-brown colour are procured; they have the appearance of hexagonal tables. This is the deutiodide of phosphorus; it melts at about $115^\circ F.$, and decomposes by water, yielding hydriodic acid when heated with a small quantity of liquid.

In operating on quantities in the proportions of 1 equivalent of phosphorus and 1 of iodine, crystals of protiodide are obtained, and excess of phosphorus remains in the liquid.

With 5 equivalents of iodine and 2 of phosphorus, protiodide crystallizes first, and afterwards deutiodide, which gives the following equation: $5I + 2Ph = I^2Ph + I^3Ph$.

With 4 and 5 equivalents of iodine and 1 of phosphorus, iodine is at first deposited, then crystals of deutiodide, I^2Ph .

By employing sulphuret of carbon as a solvent, M. Corenwinder was able to obtain several other compounds, as chloride of phosphorus and sulphuret of phosphorus, &c., in crystals.—*L'Institut*, 7 Août, 1850.

DESCRIPTION OF SOME NEW MINERALS FROM NORWAY.

BY M. P. H. WEIBYE.

Tritomite.—This mineral was found in the island of Lamoe, near Brevig, in isolated crystals, disseminated in a coarse-grained syenite, and accompanied by leucophane, mosandate, catapleite, &c.

The crystals are regular tetrahedrons, the faces of which are dull and covered with a reddish-brown crust; the fracture is conchoidal, without cleavage, and the lustre is vitreous and metallic. This mineral is very harsh, its colour is a deep brown, and its powder is yellowish-grey; it is opaque or translucent only on the edges. Its hardness is between that of felspar and apatite, and its density is 4.16 to 4.66.

Before the blowpipe tritomite whitens, exfoliates, and splits,

sometimes flying into fragments. Heated in a tube it loses water, which acts as weak hydrofluoric acid. With borax it gives a glass of a reddish-brown colour, which decolorizes on cooling. Reduced to powder it is attacked by hydrochloric acid, with the evolution of chlorine, and the separation of gelatinous silica.

M. Berlin obtained the following as the results of the analysis of this mineral, which, however, on account of the rarity of the mineral, are to be considered as merely approximative :—

Silica.	20.13
Oxide of cerium	40.36
— of lantium	15.11
Lime	5.15
Alumina	2.24
Yttria	0.46
Magnesia	0.22
Soda	1.46
Protoxide of iron	1.83
Manganese, copper, tin and tungsten.	4.62
Loss by calcination	7.86
	99.44

From the analysis, this mineral appears to be a hydrated tribasic silicate of cerium, lantium, and lime.

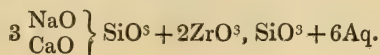
Catapleite.—This mineral accompanies the preceding. It exists in imperfect crystals, consisting of a prism of about 120° , terminated by an oblique base, also inclined at about 120° . Sometimes it exhibits indications of several vertical faces. Cleavage perfect according to the base. Fracture splintery. Dull or very little lustre, even when fractured. Colour light yellowish-brown, powder isabella-yellow; opaque or a little chatoyant. Hardness resembling that of felspar; density 2.8.

This mineral readily fuses into a white enamel; it dissolves with difficulty in borax, forming a colourless glass. A solution of cobalt renders it blue. Reduced to powder it is readily decomposed by hydrochloric acid, but does not gelatinize.

Its analysis gave M. Sjögren the following results :—

	I.	II.
Silica.	46.83	46.52
Zinconia.	29.81	29.33
Alumina.	0.45	1.40
Soda	10.83	10.06
Lime	3.61	4.66
Protoxide of iron	0.63	0.49
Water.	8.86	9.05
	101.02	101.51

Its composition may be represented by the formula—



Atheriastite.—This mineral was long confounded with paranthine; it is found in an abandoned iron mine at Naes, near Arendal, accompanied with black garnet and keilhauite in a granitic rock.

Its primary form is an octohedron with a square base, the adjacent faces of which form angles of about 135° ; this octohedron is accompanied by two square prisms. The crystals are short and thick, and the angles and edges are usually rounded. Cleaves readily according to the second square prism. Fracture uneven and splintery, dull or chatoyant. Colour pale green, powder greenish gray; opaque.

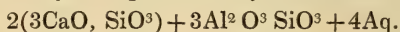
It swells before the blowpipe, and readily fuses into a deep brown glass. Hydrochloric acid attacks it very feebly.

By M. Berlin's analysis it yielded—

Silica	38·00
Alumina	24·10
Lime	22·64
Magnesia	2·80
Protoxide of iron	4·82
Protoxide of manganese ..	0·78
Water	6·95

100·09

If a part of the iron be supposed to be in the state of peroxide, this composition may be represented by the formula—



Eudnophite.—This mineral was found in the syenite of the island of Lamoe, with the two first minerals above described. Its crystals are very rare; they are derived from a right rhombic prism, giving a prism of about 130° , terminated by a bevil, and truncated at the acute edges. Cleavage perfect according to the base; there also exists a more difficult one according to the diagonal planes. Fracture even, slightly splintery. The faces are dull or a little brilliant, lustre slightly pearly when fractured. Colour white, passing to gray or brown; powder white. Fragments translucent or transparent. Hardness between that of felspar and apatite. Density 2·27.

Before the blowpipe this mineral fuses into a colourless glass. Reduced to powder, it is decomposed by hydrochloric acid with the formation of gelatinous silica.

It has been analysed by M. de Bock (I.), and by M. Berlin (II.).

	(I.)	(II.)
Silica	54·93	55·06
Alumina	23·59	23·12
Soda	14·06	14·06
Water	8·29	8·16
	100·87	100·40

These results agree exactly with the formula of analcime, $3\text{NaO}, 2\text{SiO}^3 + 3(\text{Al}^2\text{O}^3, 2\text{SiO}^3) + 6\text{HO}$, and may seem to indicate

that eudnophite is a dimorphous species of analcime.—Pogg. *Ann.* tome 79; *Bibliothèque Universelle*, Juin 1850.

ON THE HYOSKLERITE OF ARENDAL.

BY M. C. RAMMELSBERG.

Hyposklerite is a mineral of a felspathic appearance, which has been described by M. Breithaupt. An analysis recently made by M. Hermann seems to separate it completely from the felspars. M. Rammelsberg has repeated the analysis with a specimen which he received from M. Breithaupt himself. He did not find in it any traces of cerium and lantanum indicated by M. Hermann, and his results agree exactly with the composition of an albite, mixed with about 5 per cent. of pyroxene, a supposition which is supported by the deep blackish green colour interspersed through some parts of the hyposklerite. It appears therefore that this mineral should not constitute a distinct species.—*Ibid.*

ON THE EXISTENCE OF IODINE IN BEET-ROOT.

After the discovery of the existence of so important a substance as iodine in so many bodies, M. Lamy thought it might be interesting to state how he had ascertained its presence in the beet-root of the Grand Duchy of Baden.

In November last M. Lamy received from M. L. Lintz, chemist at the sugar manufactory of Waghausel in the Grand Duchy, a specimen of beet-root potash for examination, thinking that it contained iodine.

Some fragments were accordingly dissolved in distilled water and saturated with nitric or sulphuric acid; the solution was of a yellow colour and exhaled the odour of iodine; by the addition of solution of starch, it became of an intense blue colour, which disappeared by heat and reappeared on cooling.

After frequently repeating this experiment, and being certain of the existence of an alkaline iodide in the potash of Waghausel, M. Lamy examined successively the various products of the manufacture of sugar of this locality, beginning with the saline matter, molasses, then taking the refined sugar, unrefined sugar, and *cossettes*, or beet-root cut into small parallelopipeds and dried.

The saline matter was treated with hot water as long as it dissolved anything; the aqueous solution was evaporated to dryness, and the residue treated with highly rectified alcohol, the solution being evaporated to dryness; the residue was divided into two portions; one of these was treated with sulphuric acid and solution of starch, and the other was tried by M. Reynoso's process; in both cases the existence of iodine was evident.

The ash of the molasses was boiled in distilled water; a portion of the filtered liquor, saturated with nitric or sulphuric acid, gave, like the potash, a fine blue colour on the addition of solution of starch; another portion, on spontaneous evaporation, left a crystallized residue which was treated with hot alcohol of 40 degrees

to dissolve the iodide and separate the foreign salts insoluble in it. The alcoholic solution was evaporated to dryness, and the residue treated with water yielded a solution which gave a deep blue colour with starch. This colour was very permanent, disappeared on being heated and reappeared on cooling.

The same treatment was followed with sugar unrefined and refined, but they gave not the least trace of iodine; the cossettes, on the contrary, contained this substance; the experiment was several times repeated and always with the same result.

The author examined the beet-root from a manufactory in the neighbourhood of Versailles, but he discovered no trace of iodine in it. As the manufactory of Waghausel is of great extent, M. Lamy thinks it probable that all the beet-root used in it may not contain iodine, and as salts of iodine are not uncommon in the salt-springs of Germany, he inquires, without attempting to decide, whether the presence of iodine may not be derived from the assimilation of the salts of iodine.—*Journ. de Pharm. et de Chim.*, Juillet, 1850.

ELECTRO-MAGNETISM AS A MOTIVE POWER.

Professor Page, in the lectures which he is now delivering before the Smithsonian Institution, states that there is no longer any doubt of the application of this power as a substitute for steam. He exhibited the most imposing experiments ever witnessed in this branch of science. An immense bar of iron, weighing 160 lbs., was made to spring up by magnetic action, and to move rapidly up and down, dancing like a feather in the air, without any visible support. The force operating upon the bar he stated to average 300 lbs. through ten inches of its motion. He said he could raise this bar 100 feet as readily as ten inches, and he expected no difficulty in doing the same with a bar weighing one ton, or a hundred tons. He could make a pile-driver, or a forge hammer, with great simplicity, and could make an engine with a stroke of six, twelve, twenty, or any number of feet. The most beautiful experiment we ever witnessed was the loud sound and brilliant flash from the galvanic spark, when produced near a certain point in his great magnet. Each snap was as loud as a pistol; and when he produced the same spark at a little distance from this point, it made no noise at all. This recent discovery is said to have a practical bearing upon the construction of an electro-magnetic engine. He then exhibited his engine of between four and five horse-power, operated by a battery contained within a space of three cubic feet. It looked very unlike a magnetic machine. It was a reciprocating engine of two feet stroke, and the whole engine and battery weighed about one ton. When the power was thrown on by the motion of a lever, the engine started off magnificently, making 114 strokes per minute; though when it drove a circular saw, ten inches in diameter, sawing up boards an inch and a quarter thick into laths, the engine made but about 80 strokes per minute. The force operating upon this great cylinder throughout the whole motion of two feet was stated to be 600 lbs. when the engine was moving very slowly; but he had not been able to ascer-

tain what the force was when the engine was running at a working speed, though it was considerably less. The most important and interesting point, however, is the expense of the power. Professor Page stated that he had reduced the cost so far that it was less than steam under many and most conditions, though not so low as the cheapest steam-engines. With all the imperfections of the engine, the consumption of 3 lbs. of zinc per day would produce one-horse power. The larger his engines, contrary to what has been known before, the greater the economy. Professor Page was himself surprised at the result. There were yet practical difficulties to be overcome, the battery has yet to be improved, and it remains yet to try the experiment on a grander scale—to make a power of 100 horse, or more.—*National Intelligencer* (American paper).

METEOROLOGICAL OBSERVATIONS FOR JULY 1850.

Chiswick.—July 1. Cloudy: clear. 2. Fine. 3. Rain: cloudy and boisterous. 4. Heavy rain. 5. Fine: clear. 6. Fine: overcast. 7. Rain. 8. Very fine. 9. Cloudy: showery. 10, 11. Very fine. 12. Foggy: overcast. 13, 14. Overcast and fine. 15. Very fine: sultry: clear. 16. Very fine: cloudy. 17. Slight haze: very fine: overcast: rain. 18. Heavy rain: sultry: cloudy and mild. 19. Rain. 20. Overcast. 21. Cloudy and fine. 22, 23. Very fine. 24. Cloudy: rain at night. 25. Heavy showers. 26. Fine: windy: cloudy: rain. 27. Rain: showery. 28. Slight showers. 29. Cloudy: very fine: quite clear. 30. Cloudy: clear at night. 31. Slight haze: fine.

Mean temperature of the month 61°·91

Mean temperature of July 1849 62 ·29

Mean temperature of July for the last twenty-three years . 63 ·23

Average amount of rain in July 2·38 inches.

Boston.—July 1. Cloudy: rain P.M. 2. Fine. 3. Cloudy. 4. Cloudy: rain with thunder and lightning P.M. 5. Cloudy. 6. Cloudy: rain A.M. and P.M. 7. Rain. 8. Fine. 9. Cloudy: rain P.M. 10—12. Cloudy. 13—15. Fine. 16. Fine: rain A.M. and P.M. 17. Fine. 18. Calm: rain P.M. 19. Rain: rain A.M. and P.M. 20, 21. Cloudy. 22, 23. Fine. 24. Cloudy: rain early A.M. 25, 26. Cloudy: rain A.M. and P.M. 27. Rain: rain A.M. and P.M. 28. Cloudy: rain A.M. and P.M. 29, 30. Cloudy. 31. Fine: rain A.M. and P.M.

Applegarth Manse, Dumfries-shire.—July 1. Heavy rain at night: showers. 2. Showers all day and wind. 3. Showers: fair P.M.: wind. 4. Showers: cleared P.M. 5. Heavy showers: fine P.M. 6. Rain all day. 7. Fine A.M.: a few drops P.M. 8. Very fine all day. 9. Showers nearly all day. 10. Very fine all day. 11. Very warm: slight drizzle. 12. Very warm: thunder. 13. Very warm: oppressive. 14. Very warm: close. 15. Very warm: bright. 16. Very warm: slight showers: thunder. 17. Very warm: thunder. 18. Warm: dull: hazy: thunder. 19. Warm: shower early. 20. Warm still and pleasant. 21. Warm and fine: cloudy P.M. 22. Warm: a few drops of rain. 23. Warm: sultry: thunder. 24. Heavy rain. 25. Fair A.M.: wet all rest of the day. 26. Showery all day. 27. Very slight drizzle. 28. Fair: warm. 29. Fair and very fine. 30. Fair and warm. 31. Shower early: fair P.M.

Mean temperature of the month 59°·4

Mean temperature of July 1849 57 ·0

Mean temperature of July for the last twenty-eight years ... 58 ·1

Average rain in July for twenty years 3·91 inches.

Sandwick Manse, Orkney.—July 1. Showers. 2. Clear: drops. 3. Rain. 4. Fine: clear. 5. Cloudy: clear. 6. Clear: fine. 7. Clear: showers. 8. Showers: hail. 9. Bright: clear. 10. Cloudy: fine. 11. Fog: rain. 12. Cloudy. 13. Cloudy: fine. 14, 15. Fine: very fine. 16. Fine: very fine: hot. 17. Drops: showers: fog. 18. Cloudy: fine. 19. Cloudy: fine: cloudy. 20. Cloudy: fine: fog. 21. Bright: hazy. 22. Hazy: fine. 23. Bright: fine. 24. Clear: fine. 25. Clear: fine: hot: fog. 26. Cloudy: hot: drizzle. 27. Fog: damp. 28. Cloudy: fog. 29, 30. Cloudy. 31. Bright: clear.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; by the Rev. W. Dunbar, at Applegarth Manse, DUMFRIES-SHIRE; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

[illegible]

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[THIRD SERIES.]

OCTOBER 1850.

XXIX. *Remarks on the Forces experienced by inductively Magnetized Ferromagnetic or Diamagnetic Non-crystalline Substances.* By WILLIAM THOMSON, *Professor of Natural Philosophy in the University of Glasgow* *.

THE remarkable law laid down by Faraday in his Memoir on the Magnetic Condition of all Matter, *that a small portion of diamagnetic matter placed in the neighbourhood of a magnet experiences a pressure urging it from places of stronger towards places of weaker force*, is a simple conclusion, derived from the mathematical solution of the problem of determining the action experienced by a small sphere of matter magnetized inductively, and acted upon in virtue of its induced magnetism. Without entering upon the analytical investigation, which will be found in a paper “On the Forces experienced by small Spheres under Magnetic Influence; and on some of the Phænomena presented by Diamagnetic Substances†,” I shall, in the present communication, state and explain briefly the result, and point out some remarkable inferences which may be drawn from it.

1. Let P be any point in the neighbourhood of a magnet, and let P' be a point at an infinitely small distance, which may be denoted by a , from P . Let R denote the force which a “unit north pole‡” if placed at P would experience, or, as it is

* Communicated by the Author.

† Cambridge and Dublin Mathematical Journal, May 1847.

‡ That is, the end of an infinitely thin uniformly and longitudinally magnetized bar of “unit strength,” which is repelled *on the whole from the north* by the magnetism of the earth; “unit strength” being defined by the following statement:—

If two infinitely thin bars be equally, and each uniformly and longitudinally magnetized, and each uniformly and longitudinally
Phil. Mag. S. 3. Vol. 37. No. 250. Oct. 1850. R

called, "the resultant magnetic force at P;" and let R' denote the same with reference to P' . Then, if a small sphere of any kind of non-crystalline homogeneous matter, naturally unmagnetic, but susceptible of magnetization by influence, be placed at P, it will experience a force of which the component along PP' is

$$\mu \cdot \sigma \cdot \frac{1}{2} \frac{R'^2 - R^2}{a},$$

where σ denotes the volume of the sphere, and μ a coefficient depending on the nature of the substance. This coefficient, μ , has a value a little less than $\frac{3}{4\pi}$ for soft iron, and it has very small positive values for all ferromagnetic substances containing little or no iron.

2. If it be true, as I think it must be, that the forces experienced by diamagnetic substances are occasioned by the influencing magnet magnetizing them inductively*, and acting upon them when so magnetized, according to the established laws of the mutual action of two magnets, the preceding result will hold for all non-crystalline matter; and to apply it to a diamagnetic substance it will be only necessary to give μ a negative value.

3. To interpret this result, we may remark, that, by the elementary principles of the differential calculus as applied to the variation of a quantity depending on the position of a point in space, it may be shown that the fraction $\frac{R'^2 - R^2}{a}$ is greater when the point P' is chosen in a certain determinate direction from P than in any other; that it is of equal absolute value, but negative, if P' be chosen in the opposite direction; and

dinally, magnetized, and if, when an end of one is placed at a unit (an inch, for example) of distance from an end of the other, the mutual force between these ends is unity; the magnetic strength of each is unity. The force R , defined in the text, is of course equal and opposite to the force that a "unit south pole" would experience if placed at P.

* This most natural explanation of the phenomena which he had discovered is suggested by Faraday in his original paper on the subject, and it is confirmed by the researches of subsequent experimenters, especially those of Reich and Weber, who have made experiments to show that a diamagnetic substance, under the influence of two magnets, will act upon one in virtue of the magnetization which it experiences from the other. The extreme feebleness of the polarity induced in diamagnetic substances is proved by Faraday in a series of experiments forming the subject of his last communication to the Royal Society; in which an attempt is made, by very delicate means, to test the induced current in a helix due to magnetization or demagnetization of a diamagnetic substance which it surrounds, but only negative results are obtained.

that it vanishes if P' be in a plane through P at right angles to the line of those two directions. Hence it follows that the resultant force upon the small sphere is along that line, in one direction or the other, according as μ is positive or negative, and accordingly we draw the following conclusions:—

(1.) A small ferromagnetic sphere, in the neighbourhood of a magnet, will experience a force urging it *in that direction in which the “magnetic force” increases most rapidly.*

(2.) A small diamagnetic sphere, in the neighbourhood of a magnet, will experience a force urging it *in that direction in which the magnetic force decreases most rapidly.*

(3.) The absolute magnitude of the force in any case in which the distribution of magnetic force in the neighbourhood of the magnet is known, is the value which the expression in § 1 obtains when we give $\frac{R'^2 - R^2}{a}$ the value found by means of the

differential calculus, for a point P' at an infinitely small distance PP' in the direction of the most rapid variation of the magnetic force from P , the actual position of the ball.

4. It is deserving of special remark, that the direction of the force experienced by the ball has no relation to the direction of the lines of magnetic force through the position in which it is placed. The mathematical investigation thus affords full confirmation and explanation of the very remarkable observation made by Faraday (§ 2418), that a small sphere or cube of inductively magnetized substance is in some cases “urged along, and in others obliquely or directly across the lines of magnetic force.” It is in fact very easy to imagine, or actually to construct, arrangements in which the resultant force experienced by a ball of soft iron, or of some diamagnetic substance, is perpendicular to the lines of the magnetizing force. For instance, if a ball of soft iron be placed symmetrically with respect to the two poles of a horseshoe magnet, and at some distance from the line joining them, it will be urged towards this line, in a direction perpendicular to it, and consequently perpendicular to the lines of magnetizing force in the space in which it is situated; and a ball of bismuth, or of any other diamagnetic substance, similarly situated, would experience a force in the contrary direction. Or again, if a ball of any substance be placed in the neighbourhood of a long straight galvanic wire, it will be urged towards or from the wire (according as the substance is ferromagnetic or diamagnetic) in a line at right angles to it, and consequently cutting perpendicularly the lines of force, which are circles with their centres in the wire and in planes perpendicular to it.

5. The preceding conclusions enable us to define clearly the

sense in which the terms "attraction" and "repulsion" may be applied to the action exerted by a magnet on a ferromagnetic and a diamagnetic body respectively. A small sphere of ferromagnetic substance, placed in the neighbourhood of a magnet, experiences in general, a force; but the term *attraction*, according to its derivation, means a *force towards*; and if we apply it in any case, we must be able to supply an object for the preposition. Now, in this case the force is towards places of stronger "magnetic force;" and hence the action experienced by a ferromagnetic ball may be called an *attraction* if we understand *towards places of stronger force*. Places of stronger force are generally nearer the magnet than places of weaker force, and hence small pieces of soft iron are generally urged, on the whole, towards a magnet (in consequence of which no doubt the term "attraction" came originally to be applied): but, as will be seen below, this is by no means universally the case; balls of soft iron being, in some cases, actually repelled from the influencing magnet; and the term "attraction" can only be universally used with reference to ferromagnetic substances, on the understanding that it is towards places of stronger force. The term "repulsion," the reverse of "attraction," may, according to the same principles, be applied universally to indicate the force with which a small diamagnetic sphere is urged towards places of weaker force, or *repelled from places of stronger force*.

6. The following passage, containing a statement of principles on some of which Faraday himself lays much stress, but which have not I think been sufficiently attended to by subsequent experimenters, is quoted from the article in the *Mathematical Journal* already referred to.

7. "The result obtained above affords the true explanation of the phænomenon observed by Faraday, that a thin bar or needle of a diamagnetic substance, when suspended between the poles of a magnet, assumes a position across the line joining them. For such a needle has no tendency to arrange itself across the lines of magnetic force; but, as will be shown in a future paper, if it be very small compared with the dimensions and distance of the magnet (as is the case, for instance, with a bar of any ordinary dimensions, subject only to the earth's influence), the direction it will assume, when allowed to turn freely about its centre of gravity, will be that of the lines of force, whether the material of which it consists be diamagnetic, or magnetic matter such as soft iron: but Faraday's result is due to the rapid decrease of magnetic intensity round the poles of the magnet, and to the length of the needle, which is considerable compared with the

distance between the poles of the magnet; and is thus explained by the discoverer himself. (§ 2269) ‘The cause of the pointing of the bar, or any oblong arrangement of the heavy glass, is now evident. It is merely a result of the tendency of the particles to move outwards, or into the positions of weakest magnetic action.* The joint exertion of the action of all the particles brings the mass into the position which, by experiment, is found to belong to it.’”

8. It may be added to this, that the tendency of a bar, whether of ferromagnetic or of diamagnetic substance, in a uniform field of magnetic force, to take the direction of the lines of force, depends on the effect of the mutual action of the parts in altering the general magnetization of the bar, and is consequently so excessively feeble for any known diamagnetic substance that the most delicate experiments would in all probability fail to render it sensible†.

9. Faraday’s law, stated at the commencement of these remarks, may be illustrated by some very curious although extremely simple experiments, which I shall now describe briefly‡.

10. The special apparatus required is merely a long light arm (I have used one about four feet in height; but a much shorter rod, if suspended by a finer or by a longer torsion-thread, would have answered equally well) suspended from a “torsion-head” by means of a very fine wire, or thread of unspun silk fibres attached to it near its middle; and a case round it adapted to prevent currents of air from disturbing its equilibrium, but allowing it sufficient angular motion in a horizontal plane. A small ball of soft iron is attached to one end of the arm (or hung from it by a fine thread, which, for the sake of stability in many of the experiments, as for instance, experiments 2 and 3 described below, must not be too long), and a counterbalance is adjusted near the other end so as to make the arm horizontal. If only a small angular motion be allowed to the arm, the path of the ball will be sensibly straight, and we may consider that, by the arrangement

* ‘The extreme feebleness of the diamagnetic action, on account of which any small sphere or cube of the matter will experience very nearly the same force as if all the rest were removed, seems fully to justify this explanation.’

† A very brief communication on this subject was laid before the British Association at the meeting of 1848, and is published in the Report for that year, under the title “On the Equilibrium of Magnetic or Diamagnetic Bodies of any form, under the Influence of Terrestrial Magnetic Force.”

‡ These experiments were shown, in illustration of lectures on magnetism in the Natural Philosophy Class in the University of Glasgow, during the Session 1848-49.

which has been described, the ball is allowed to move with great freedom in a straight line, but prevented from all other motion.

11. In making the experiments described below, it is convenient to have two stops so arranged that the motion of the arm may be kept within any desired limits, and manageable in such a way, that by means of them the arm may be rapidly brought to rest in any position. In general, before commencing an experiment, the arm ought to be brought to rest near one end of its course, and kept pressing very slightly upon one of the stops by the torsion of the wire, which may be suitably adjusted by the torsion-head, and the other stop ought to be pushed away, so as to leave the arm free to move in one direction.

12. *Experiment 1.*—Place a common bar-magnet with either pole, the south, for instance, near the ball of soft iron in its line of motion, but on that side towards which it is prevented from moving by the stop. Taking another bar-magnet of considerably greater strength than the former, bring its north pole gradually near the fixed south pole of the other, in the continuation of the line of motion of the iron ball. When this north pole reaches a certain position the arm will cease to press on the stop, and if we push the north pole a little nearer still, the arm will altogether leave the stop and take a position of equilibrium, in which, after it is steadied (as may easily be done by means of the stops), it will remain stable, although the stops be removed entirely. If, by means of one of the stops, the ball be pushed to any distance farther from the magnets than this position of stable equilibrium, it will return towards it when left free. If it be drawn a little nearer by means of the other stop, and, when left for a few seconds, it be found to continue pressing upon the stop, then, when the stop is removed, the ball will return to that position of stable equilibrium. If, however, it be very slowly drawn still nearer the magnets, when it reaches a certain position it will cease to press on the stop; and if after this it experience the slightest agitation, or if it be drawn any nearer, it will leave the stop and move up till it strikes the nearer magnet, in contact with which it will almost immediately come to rest. It thus appears that there is a position of unstable equilibrium for the ball between the former stable position and the nearer magnet. It is easy to arrange the torsion-head so that the torsion of the suspending-thread or wire may have as little effect as we please, by finding, by successive trials, either of these positions of equilibrium, subject to the condition that, when the magnets are removed, the torsion would not sensibly disturb the arm from the position so found.

13. After the explanations which have been given above, it is scarcely necessary to point out that the position of unstable equilibrium, determined in this experiment, is a point where the magnetizing force due to the south pole is destroyed by that of the more distant but more powerful north pole; and that the position of stable equilibrium is one where the excess of the magnetizing force due to the north pole, above that which is due to the less powerful south pole, has a maximum value with reference to points in the continuation, through the less powerful pole, of the line joining the two poles. If the poles were mathematical points, and the bars so long that their remote ends could produce no sensible action on the ball, the position of unstable equilibrium would of course be such that *its distances from the two poles would be directly as the square roots of the strengths of the magnets*; and, by the solution of a most simple "maximum problem," it may be shown that the stable position would be such that *its distances from the poles would be directly as the cube roots of the strengths*.

14. *Experiment 2.*—Place two equal bar-magnets symmetrically with reference to the line of motion, with similar poles at equal distances on two sides, in a perpendicular to this line, and, to make the best arrangement, let the lengths of the magnets be in the continuations of the lines joining their poles. Operating by means of the stops, in a manner similar to that described for the preceding experiment, it is readily ascertained that there are two positions of stable equilibrium for the ball at equal distances on two sides of the line joining the poles, and that the middle point of this line is a position of unstable equilibrium.

15. Here, again, the explanation is obvious. The positions of stable equilibrium being such that, with reference to points in the line of motion of the ball, the magnetizing force due to the two similar poles may be a maximum, are readily found to be at distances $\frac{a}{2\sqrt{2}}$ on the two sides of the line joining the poles (the length of this line being denoted by a), if these be mathematical points, and if the lengths of the bars be so great that the distant poles produce no sensible effects.

16. *Experiment 3.*—Hold a common horseshoe-magnet with the line joining its poles perpendicular to the line of motion of the ball, and, by a suitable management of the stops and of the torsion-head, the existence of a force urging the ball perpendicularly across the "lines of force" towards the middle point of the line joining the poles, may be easily made manifest.

17. *Experiments on diamagnetic substances, and on ferro-*

magnetic substances of feeble inductive capacity.—The phenomena discovered by Faraday relative to the action of magnets on substances not previously known to be susceptible of magnetic influence may be exhibited with great ease by means of the apparatus described above. Small balls of the substances to be experimented upon may be hung from one end of the balance (the ball of soft iron being of course removed) by fine threads of sufficient length to allow the arm, which may be of any substance containing no iron, to be out of reach of any sensible influence from the magnet employed. There is in these cases no difficulty, regarding the length of the suspending-thread, of the kind noticed above with reference to soft iron, as the magnetic forces experienced are never strong enough to produce lateral instability (that is, a want of stability in the line of motion), even with the lightest of the substances experimented on, unless the suspending thread be far longer than is necessary. In the experiments I have made, the threads bearing the small balls have not been more than four or five inches long. The diameters of the balls have been from a quarter of an inch to an inch, or an inch and a half. Instead of simple bar-magnets of steel, which are not powerful enough to be convenient for these experiments, I have used a bar electro-magnet of very moderate power, consisting of a helix and soft iron core. This core is a cylinder of about an inch in diameter and a foot and a half long, *with round ends* (nearly hemispherical), which, when the core is in its central position, extend about an inch beyond the helix on each side. By these means the repulsion of balls of diamagnetic substance, and the attraction of very feebly ferromagnetic substances, may be shown with great facility.

18. For example, I may mention that I have hung a small apple, whole, by a thread three or four inches long, and putting it at first at rest, pressing slightly (in virtue of torsion produced by the torsion head mentioned above) upon one end of the soft iron core previously to the excitement of the electro-magnet, I have found that as soon as the galvanic current is produced, the apple is repelled away; and, by pushing forward the soft iron core, I have chased it across the field through a space of four or five inches.

19. I have also used the same apparatus to show that a body which is feebly attracted in air is repelled when immersed below the surface of a sufficiently strong solution of sulphate of iron in a small trough, so arranged that when, by the force of torsion, the body immersed in the liquid is made to press on a side of the trough, the electro-magnet may be placed with one end of its core pressing on the outside of the

trough, close to the point where it is pressed upon by the body within. Using small glass balls (which, when empty, exhibit no sensible effects of the influence of the magnet), the magnetic conditions of different liquids filling them may be easily tested. Faraday's beautiful experiments on the relative magnetic capacities of solutions of sulphate of iron of different strengths, or rather, other experiments to illustrate the same principles, may be performed in an extremely convenient manner, by filling a glass ball of this kind with a solution, hanging it from one end of the arm, and, by a suitable adjustment of the weight at the other, immersing it below the surface of another solution contained in the trough. I have found that whenever the difference of the strengths of the two solutions was considerable, the ball immersed was attracted or repelled by the external magnet, according as the solution contained in the ball was stronger or weaker than the solution surrounding it.

*On the stability of small inductively magnetized bodies
in positions of equilibrium.*

20. In the paper published in the Mathematical Journal (referred to above), I pointed out that a small ball of either ferromagnetic or diamagnetic substance placed in the neighbourhood of a magnet, and not acted upon by any non-magnetic force, is in *equilibrium* if it be in a situation where the "resultant force" (that which was denoted by R) is either a maximum or minimum, or "stationary" in value; that a diamagnetic ball is in *stable equilibrium* if, and not in stable equilibrium unless, it be situated where the force R is a minimum in absolute value; and that "if there be any point external to the magnet, at which the resultant force has a maximum value, it would be a position of stable equilibrium for a small bar of soft iron, and any other position is essentially unstable." Shortly after the publication of that paper, I succeeded in proving that the resultant force cannot be an absolute maximum at any point external to a magnet, and consequently that no position of stable equilibrium for a ferromagnetic ball, perfectly free from all constraint, can exist. I have very recently found that there may be points where the resultant force is an absolute minimum without being zero; and therefore there may be positions of stable equilibrium for a diamagnetic ball not included in the case of the force vanishing, noticed in the previous paper. This case however affords the simplest illustration that can be given of that most extraordinary fact, that a solid body may be repelled by a magnet, or magnets, into a position of stable equilibrium. If,

for instance, we take the arrangement (described for Exp. 2 above) of two bar-magnets, fixed with similar poles near one another, we have obviously between these poles a point where the resultant force vanishes, and towards which consequently a small diamagnetic ball placed anywhere sufficiently near it would be repelled. It is easily shown that, actually under the action of gravity, a ball of diamagnetic substance would be in stable equilibrium a little below this position, without any external support or constraint whatever, if only the magnets were strong enough. It is, however, extremely improbable that any attempt to realize this by experiment will succeed, since, even in the most favourable cases, no diamagnetic repulsion upon a solid has yet been obtained which at all approaches in magnitude to the weight of the body. Still we must consider that a true theoretical solution of the celebrated physical problem * suggested by "Mahomet's coffin" has been obtained, which is not the least curious among the remarkable consequences of Faraday's magnetic discoveries.

On the relations of Ferromagnetic and Diamagnetic Magnetization to the Magnetizing Force.

21. In the mathematical investigation by which the result stated above was obtained, it is assumed that the magnetization of the substance of the ball in each case is proportional to the magnetizing force (although this assumption may of course be avoided by merely supposing μ to have a value varying with the force, which will not affect either the investigation or the form of the result). It appears to me very probable that this assumption is correct for all known diamagnetic substances, and for homogeneous feebly ferromagnetic substances; since it is equivalent to an assumption that inductive magnetization of a substance does not impair or in any way alter its susceptibility for fresh magnetization by means of another magnet brought into its neighbourhood. This opinion cannot however at present be regarded but as a mere conjecture, being as yet unsupported by experiment. It is indeed directly op-

* It is I believe often thought that this problem is solved in the experiment in which a needle is attracted into a galvanic helix held with its axis vertical; but I have convinced myself that the needle always touches somewhere on the sides of the tube (if there be one round it) or on the wire of the helix: and I have also ascertained that, when a powerful helix is used with, in place of the needle, a tin-plate cylinder, even if it be very little less in diameter than the inner cylindrical surface of the helix, there is never stable equilibrium without contact between them. The phenomenon of a solid body, hovering freely in the air, in stable equilibrium, without any external support or constraint, has never, I am convinced, been witnessed as the result of any electrical or magnetical experiment.

posed to the following conclusion to which M. Plücker arrives, from some of his experimental researches :—"J'ai déduit de là cette loi générale, savoir : que le diamagnétisme décroît plus vite que le magnétisme quand la force de l'aimant diminue, ou quand la distance des pôles augmente"* : but many of the curious phænomena from which M. Plücker was led to this conclusion, and which he adduces in confirmation of it, do not appear to me to support it, but rather to be connected with the peculiar magneto-inductive properties of crystalline or quasi-crystalline structure which he discovered subsequently †; and with respect to those which appear at first sight really to support it, I have conjectured that they may admit of explanation solely on the principle expressed in Faraday's law, quoted at the commencement of these remarks. Thus, the experiments upon a watch-glass containing mercury, placed at different distances from a magnet, which show that the resultant force experienced by the watch-glass, in virtue of its own magnetization as a ferromagnetic substance, and the contrary magnetization of the diamagnetic mercury, is sometimes increased by removing the whole to a slightly greater distance from the magnet, do not prove that when the magnetizing force is diminished the induced magnetization of the mercury is diminished by a greater fraction of its former amount than that of the watch-glass, but are most probably to be explained by the circumstance that the "field of force" occupied by the mercury and watch-glass, when removed a very short distance, is such that the mean value of the differential coefficient of the square of the force, with reference to co-ordinates parallel to the direction of motion of the watch-glass, is greater than the mean value of the same function, through the field occupied when the watch-glass is in contact with the magnet. It is of course impossible to give more than a general explanation such as this without some specific knowledge of the distribution of magnetic force in the neighbourhood of the actual magnet employed; but the phænomena described by M. Plücker in this case are undoubtedly of a kind that might be anticipated if a vertical bar-magnet be

* Quoted from a paper in the French *Annales de Chimie et de Physique*, June 1850, bearing the title, "Sur le Magnétisme et le Diamagnétisme : par M. Plücker." This paper appears to be a *résumé* of the author's experimental researches and discoveries regarding magnetic induction, of which detailed accounts have been published in various communications to Poggendorff's *Annalen* in the course of the last two years.

† This connection is recognized by the discoverer himself, as is shown by the statement he makes at the commencement of § 4 of the paper already referred to. Yet he mentions his experiments on cylinders of charcoal as the foundation on which he establishes, as a general law, the conclusion quoted in the text.

used, especially if the upper pole, over which the watch-glass is suspended, be flat. An electro-magnet with, for core, a hollow cylinder of soft iron open at the ends, would even *repel* a small ferromagnetic body capable of moving along the axis, in some positions, and *attract* it a little further off, since there would be variations of force in this case precisely similar to those explained with reference to points in the line of motion of the ball in experiment 2.

22. The most striking experiments adduced by M. Plücker to support his hypothesis, that “diamagnetism increases more rapidly than magnetism” when the magnetizing force is increased, are those in which the force experienced by a small inductively magnetized body in a constant position is tested for different strengths of the same electro-magnet, produced by using a greater or less number of cells in the exciting battery. At the recent Meeting of the British Association in Edinburgh, I ventured to suggest *that a change in the distribution of magnetic force in the neighbourhood of the magnet, accompanying an increase or diminution in the strength of the galvanic current, might have contributed to produce some of the singular phænomena which had been observed; and that there is some considerable change in the distribution of force in the neighbourhood of an electro-magnet with a soft iron core in a state of intense magnetization when, for instance, the strength of the current is doubled, seems extremely probable when we consider that a piece of soft iron in a state of intense magnetization cannot be expected to be as open to fresh magnetization as it would be if not magnetized in the first instance.* On the same occasion I remarked, that some experiments made by Mr. Joule in connexion with his researches on changes of dimensions produced in iron bars by magnetic influence, appeared to indicate diminished inductive capacities in states of intense inductive magnetization*. At that time I was not aware of the recent experimental researches of Gartenhauser and Müller on the magnetization of soft iron; but I have since met with a Number of Poggendorff’s *Annalen* (1850, No. 3, published last April) containing an account of these researches†, which completely confirms the second part of the conjecture I had thrown out. Whether or not, however, the change in the distribution of force is of such a kind as to account for the phænomena by which M. Plücker supports the conclusion which has been quoted, it is impossible to pronounce without a complete knowledge of the circum-

* Phil. Mag. 1847, vol. xxx. pp. 76, 225. Also Sturgeon’s *Annals*, Aug. 1840.

† “Ueber die Magnetisirung von Eisenstäben durch den Galvanischen Strom; von J. Müller.”

stances. An *experimentum crucis* might be made by means of an electro-magnet without a soft iron core.

23. In one respect M. Plücker's views receive a remarkable confirmation by Joule and by Gartenhauser and Müller's experiments, if it be true that a homogeneous *diamagnetic* substance is inductively magnetizable to an extent precisely proportional to the magnetizing force, or deviating less from this proportionality than the magnetization of soft iron. For if a complex body were made up consisting of a diamagnetic substance (either solid or in powder) and an extremely small quantity of soft iron in very fine powder or filings, spread uniformly through it; a small ball of this body would, when acted upon by a feeble magnetizing force, become on the whole magnetized like a ferromagnetic, and would be urged from places of weaker towards places of stronger force. If now the magnetizing force were gradually increased, the "resultant magnetic moment" of the complex body would at first increase, then, after attaining a maximum value, decrease to zero, after which it would become "negative," or the ball would be on the whole magnetized like a diamagnetic, and would be urged from places of stronger towards places of weaker force. Such, if I mistake not, is the bearing which M. Plücker expects of any complex solid consisting of a suitable mixture of ferromagnetic and diamagnetic substances; but mere experiments on soft iron, such as those of Joule and of Gartenhauser and Müller, do not render it probable that a homogeneous feebly ferromagnetic substance, containing no iron, or only a very small quantity and that chemically combined, should have its capacity for fresh magnetization diminished by the slight magnetization which the strongest magnetizing force that could be applied would produce. If however M. Plücker's experiments be ultimately admitted as conclusive (which I think they certainly must be if those in which the position of the substance is unaltered be found to succeed with a pure electro-magnet), it would be established that the capacity for magnetic induction of a solution of sulphate of iron* (ferromagnetic), in water (diamagnetic), would diminish as the magnetizing force is increased, becoming zero with a certain force; and negative, so that the liquid would be on the whole diamagnetic, with any greater force.

Row, Gare Loch, Aug. 21, 1850.

* I assume that the sulphate of iron is in a state of complete solution, as it would be with a slight excess of acid. If the liquid be at all turbid, on account of a precipitate of oxide of iron, the phenomena observed by M. Plücker might be explained as in the case of the complex solid containing soft iron in powder spread through the mass.

XXX. *On the Diffusion of Liquids.*
By THOMAS GRAHAM, F.R.S., F.C.S.

[Continued from p. 198.]

5. *Separation of Salts of different Bases by Diffusion.*

IT was now evident that inequality of diffusion supplies a method for the separation, to a certain extent, of some salts from each other, analogous in principle to the separation of unequally volatile substances by the process of distillation. The potash salts appearing to be always more diffusive than the corresponding soda salts, it follows, that if a mixed solution of two such salts be placed in the solution phial, the potash salt should escape into the water atmosphere in largest proportion, and the soda salt be relatively concentrated in the phial. This anticipation was fully verified.

(1.) A solution was prepared of equal parts of the anhydrous carbonates of potash and soda in 5 times the weight of the mixture of water. Diffused from a small thousand-grain phial of 1·1 inch aperture, into 6 ounces of water, for nineteen days, at a temperature above 60°, it gave a liquid of density 1·0350, containing a considerable quantity of the salts. Of these mixed salts, converted into chlorides by the addition of hydrochloric acid, 9·39 grs., being treated with bichloride of platinum in the usual manner, gave 19·39 grs. of the double chloride of platinum and potassium, equivalent to 5·91 grs. of chloride of potassium; and left in solution 3·44 grs. of chloride of sodium: loss 0·04 gr. These chlorides represent 5·46 grs. of carbonate of potash and 3·12 grs. of carbonate of soda. The salts actually diffused out were therefore in the proportion of—

Carbonate of soda	36·37
Carbonate of potash	63·63
	100·00

(2.) In another similar experiment from a six-ounce phial into 8½ ounces of water, the liquid of the water-jar, after twenty-five days' diffusion, contained the two carbonates in nearly the same proportions as before, namely—

Carbonate of soda	35·2
Carbonate of potash	64·8
	100·0

(3.) A partial separation of the salts of sea-water was effected in a similar manner.

The sea-water (from Brighton) was of density 1·0265. One

thousand grs. of the liquid yielded 35·50 grs. of dry salts, of which 2·165 grs. were magnesia. The dry salts contain therefore 6·10 per cent. of that earth.

Six thousand-grain phials, of 1·1 inch aperture, were properly filled with the sea-water and placed in six tumblers, each of the last containing 6 ounces of water. Temperature about 50°. The diffusion was interrupted after eight days. The salts of the sea-water were now found to be divided as follows:—

Diffused into the tumblers	. 92·9 grs., or 36·57 per cent.
Remaining in the phials	. . 161·1 grs., or 63·43 per cent.
	254·0 100·00

Rather more than one-third of the salts has therefore been transferred from the solution phials to the water-jars by diffusion.

Of the diffused salts in the tumblers, 46·5 grs. were found to contain 1·90 gr. magnesia, or 4·09 per cent. Hence we have the following result:—

Magnesia originally in salts of sea-water	. 6·01 per cent.
Magnesia in salts diffused from sea-water	. 4·09 per cent.

The magnesia, also, must in consequence be relatively concentrated in the liquid remaining behind in the diffusion cells.

A probable explanation may be drawn from the last results of the remarkable discordance in the analysis of the waters of the Dead Sea, made by different chemists of eminence. I refer to the relative proportion of the salts, and not their absolute quantity, the last necessarily varying with the state of dilution of the saline water when taken up. The lake in question falls in level 10 or 12 feet every year, by evaporation. A sheet of fresh water of that depth is thrown over the lake in the wet season, which water may be supposed to flow over a fluid nearly 1·2 in density, without greatly disturbing it. The salts rise from below into the superior stratum by the diffusive process, which will bring up salts of the alkalies with more rapidity than salts of the earth, and chlorides, of either class, more rapidly than sulphates. The composition of water near the surface must therefore vary greatly, as this process is more or less advanced.

(4.) I may be allowed to add another experiment which is curious for the protracted immobility of a column of water which it exhibits, as well as for the separation occurring, which last may be interesting also in a geological point of view. A plain glass cylinder with a foot, 11 inches in height, and of which the capacity was 64 cubic inches, had 8 cubic inches poured into it of a saturated solution of carbonate of lime in

carbonic acid water, containing also 200 grs. of chloride of sodium dissolved. Distilled water was then carefully poured over the saline solution, so as to fill up the jar, a float being used and the liquid disturbed as little as possible in the operation. The mouth of the jar was lastly closed by a ground glass plate, and it was left undisturbed upon the mantel-piece of a room without a fire, from March 20 to September 24 of the present year, or for six months and four days. Afterwards, on removing the cover, the fluid was observed not to have evaporated sensibly, and it exhibited no visible deposit. This I was not surprised at, as no deposit appeared in a similar experiment with the jar uncovered, after the lapse of six weeks. The liquid in the former jar was now carefully drawn off by a small siphon with the extremity of both its limbs recurved so as to open upwards, in four equal portions, which may be numbered from above downwards. Equal quantities of the four strata of liquids gave the following proportions of chloride of sodium and carbonate of lime:—

	Chloride of sodium.	Carbonate of lime.
No. 1.	21.91	0.10
No. 2.	23.41	0.22
No. 3.	23.55	0.38
No. 4.	23.99	0.42

The diffusion of the chloride of sodium has therefore not yet reached complete uniformity, although approaching it, the proportion of that salt obtained from the top and bottom strata being as 11 to 12. But the diffusion of the carbonate of lime appears much less advanced, the proportion of that substance being as 1 to 4 at the top and bottom of the liquid column. The slight difference in density of the strata, it may be further remarked, must have been sufficient to preserve such a column of liquid entirely quiescent, as shown by the distribution of the carbonate of lime, during the considerable changes of temperature of the season.

Chemical analysis, which gives with accuracy the proportions of acids and bases in a solution, furnishes no means of deciding how these acids and bases are combined, or what salts exist in solution. But it is possible that light may be thrown on the constitution of mixed salts, at least when they are of unequal diffusibility, by means of a diffusion experiment. With reference to sea-water, for instance, it has been a question in what form the magnesia exists, as chloride or as sulphate; or how much exists in the one form and how much in the other. Knowing however the different rates of diffusibility of these two salts, which is nearly chloride 2 and sul-

phate 1, and their relation to the diffusibility of chloride of sodium, we should be able to judge from the proportion in which the magnesia travels in company with chloride of sodium, whether it is travelling in the large proportion of chloride of magnesium, in the small proportion of sulphate of magnesia, or in the intermediate proportion of a certain mixture of chloride and sulphate of magnesia. But here we are met by a difficulty. Do the chloride of magnesium and sulphate of magnesia necessarily pre-exist in sea-water in the proportions in which they are found to diffuse? May not the more easy diffusion of chlorides determine their formation in the diffusive act, just as evaporation determines the formation of a volatile salt—producing carbonate of ammonia, for instance, from hydrochlorate of ammonia with carbonate of lime in the same solution? We shall see immediately that liquid diffusion, as well as gaseous evaporation, can produce chemical decompositions.

6. *Decomposition of Salts by Diffusion.*

(1.) At an early period of the inquiry, a solution was diffused of bisulphate of potash, saturated at 68° and of density 1.280, from the six-ounce phial of 1.175 inch aperture, into 20 ounces of water. The period of diffusion extended to fifty days. About the middle of that period, a few small crystals of sulphate of potash, amounting probably to 3 or 4 hundredths of a grain, appeared in the diffusion cell and never afterwards dissolved away. When terminated, the liquid remaining in the solution cell was found of density 1.154; that in the water-jar 1.0326. A portion of the latter liquid gave by analysis—

Sulphate of potash . . .	20.37	} Bisulphate of potash.
Sulphate of water . . .	11.47	
Sulphate of water . . .	12.77	
	44.61	

It thus appears that the bisulphate of potash undergoes decomposition in diffusing, and that the acid diffuses away to about double the extent, in equivalents, of the sulphate of potash. This greater escape of the acid will also account for the deposition of crystals of the neutral sulphate in the solution cell.

(2.) A similar experiment was made with another double sulphate of greater stability, common potash-alum. The solution of 4 anhydrous alum in 100 water, was diffused from the six-ounce phial into 24 ounces of water, at $64^{\circ}.2$, for eight days. The quantity of salt diffused in that time amounted only to 7.48 grs. It contained 1.06 gr. alumina, which is

equivalent to 5.33 grs. of alum. The diffused salt gave off no acid vapours at 600° . We may therefore suppose the excess of salt which is diffused to be sulphate of potash. The diffusion product of alum, at 64° , appears to be—

Alum	5.33	71.26
Sulphate of potash . . .	2.15	28.74
	7.48	100.00

In a second experiment, the diffusion product amounted to 6.39 grs., of which 0.95 gr. was alumina; and it is represented by 4.77 alum and 1.52 sulphate of potash.

In connexion with the low diffusibility of the sulphate of alumina of alum, it was found that the addition of caustic potash to the alum solution, so as to convert it into an aluminate of potash, increased the diffusibility of the alumina. The diffusion product from the 4 per cent. solution of alum so treated contained 1.62 gr. of alumina in one experiment and 1.54 in another.

As alum is a salt of great stability, it presents a severe test of the influence in question. The decomposition of this double salt by diffusion was further confirmed therefore in experiments made by means of the four-ounce diffusion phials of 1.25 inch aperture, and the alteration which the salt undergoes in the process more exactly ascertained. The experiments were made at a mean temperature of $57^{\circ}.9$, and lasted seven days; the solution employed being of 4 anhydrous alum to 100 water, as before.

In three experiments, the salt diffused out amounted to 5.73, 5.80 and 5.65 grs.; of which the mean is 5.73 grs. The latter quantity gave 0.82 alumina and 3.22 sulphuric acid, which correspond to 4.11 anhydrous alum and 1.62 neutral sulphate of potash. Or, we have as the diffusion product of alum, in 100 parts—

Alum	71.73
Sulphate of potash	28.27
	100.00

This analysis corresponds closely with the diffusion product of the former experiments, which gave 71.26 per cent. of alum. The solution of alum which remains behind in the solution phials must of course acquire an excess of sulphate of alumina.

The salt, sulphate of alumina, did not appear to be decomposed when diffused alone. A four per cent. solution of the hydrated sulphate of alumina, which is manufactured at Newcastle, when diffused in the same circumstances as the preceding solutions of alum, gave 3.40 grs. of anhydrous sulphate of alumina, in which the acid was to the alumina as 2.95 equivalents of the former to 1 equivalent of the latter, or as nearly

as possible in the proportion of 3 equivalents of acid to 1 of base. As the Newcastle salt contained almost exactly half its weight of water, the 3.40 grs. of anhydrous salt diffused out are equivalent to 6.80 grs. of hydrated sulphate of alumina. The sulphate of alumina appears thus to be more diffusive than the double sulphate of alumina and potash, in the proportion of 6.80 to 5.73.

(3.) It was interesting to observe what really diffuses from the ammoniated sulphate of copper (CuO , SO^3 , $2\text{NH}^3 + \text{HO}$), and to find if the low diffusibility of that salt is attended with decomposition. The diffusion of the ammoniated sulphate of copper was therefore repeated from a 4 per cent. solution in the six-ounce solution phial, for eight days, at $64^\circ.2$. In evaporating the water of the jar afterwards, the ammoniated sulphate of copper present was necessarily decomposed, by the escape of ammonia, and a subsulphate of copper precipitated. The copper found, however, was estimated as neutral sulphate of copper. The diffusion product of two experiments may be represented as follows, in grains:—

	I.	II.
Sulphate of copper . . .	0.81	0.97
Sulphate of ammonia . .	5.46	5.53
	6.27	6.50

The abundant formation and separation of sulphate of ammonia in these experiments, prove that the ammoniated sulphate of copper is largely decomposed in diffusion.

(4.) Perhaps the most interesting result of this kind is a solution which is given of the problem of the decomposition of the alkaline sulphates by means of lime.

Solutions were prepared of $\frac{1}{2}$ per cent. of sulphate of potash and of chlorides of potassium and sodium in lime-water. Two solution phials were filled with each of these solutions, and placed for diffusion in water-jars filled with lime-water, at 49° , for seven days.

In the sulphate no deposition of crystallized sulphate of lime took place within the solution phial, while the water-jar acquired an alkaline reaction, which remained after precipitating the lime entirely by carbonic acid gas and evaporating twice to dryness. Hydrate of potash, it will afterwards appear, is an eminently diffusive salt, having double the diffusibility of sulphate of potash. The tendency of the former to diffuse enables the affinity of the lime for sulphuric acid to prevail, and the alkali is liberated and diffused away into the external atmosphere of lime-water. By the latter, hydrate of lime is returned to the solution cell and the decomposition continued. The salt diffused in the two cells amounted to 2.60 grs., of which 0.62 gr., or 23.85 per cent., was hydrate

of potash. The chlorides of potassium and sodium, on the contrary, were not sensibly decomposed.

It is known that a precipitation of sulphate of lime may occur, with a larger proportion of sulphate of potash in lime-water, in a close phial without external diffusion. As the decomposition of the sulphate of potash, in the latter case, has been referred to the insolubility of the sulphate of lime, so the decomposition in the former circumstances is referred, in a similar sense, to the high diffusibility of hydrate of potash.

7. Diffusion of Double Salts.

How is the diffusion of two salts affected by their condition of combination as a double salt? A solution of the double sulphate of magnesia and potash, in the proportion of 100 water to 4 anhydrous salt, was operated upon in the four-ounce diffusion phials of 1.25 inch aperture, with a period of diffusion of seven days, at $57^{\circ}9$ F. The diffusion product of the double salt was 8.09 and 7.81 grs. in two experiments: mean, 7.95 grs.

The constituent salts, sulphate of magnesia and sulphate of potash, were now dissolved separately, in the proportions in which they existed in the double salt, namely, 1.65 gr. anhydrous sulphate of magnesia in 100 water, and 2.35 grs. sulphate of potash in 100 water, making up together 4 parts of salts. The two solutions thus contain equivalent quantities of the different sulphates.

The separate diffusion of the sulphate of magnesia was 2.09, 2.11 and 2.40 grs. in three cells; and of the sulphate of potash, 5.83, 5.97 and 5.54 grs. in three cells; the circumstances of the experiments being the same as those of the double salt. The means of the two salts are 2.20 and 5.78 grs.; and the sum of the two means 7.98 grs. The result is, that the separate diffusion of the constituent salts is almost identical with their diffusion when combined as a double salt:—

Diffusion of the double sulphate of magnesia and potash	} 7.95 grs.
Diffusion of equivalents of sulphate of magnesia and sulphate of potash in separate cells	} 7.98 grs.

It would thus appear that the diffusibility of this double salt is the sum of the separate diffusions of its constituent salts.

It has been a question whether a double salt is formed at once when its constituent salts are dissolved together, or not till the act of crystallization of the compound salt. Equivalents of the same two sulphates, making up 4 parts, were dissolved together without heat in 100 water. Now the diffusion from this mixture, which has the composition of the preceding

solution of the double salt, exhibited notwithstanding a sensibly different result of diffusion, giving 7.28, 7.37 and 7.26 grs. in three cells; mean, 7.30 grs. The diffusion of the double salt was greater, namely, 7.95 grs. Hence a strong presumption that the mixed salts last diffused were not combined, and that the double sulphate of magnesia and potash is not necessarily formed immediately upon dissolving together its constituent salts.

In early experiments of a similar nature made upon the double salt, sulphate of copper and potash, and upon a mix- of the two sulphates newly dissolved together, a similar result was obtained. While the diffusion of the mixed salts was 25.6 grs., that of the same weight of the combined salts (the double sulphate) was 30 grs. The double salt appears more diffusible, in both cases, than its mixed constituents.

These double salts appear to dissolve in water without decomposition, although the single salts may meet in solution without combining. Hence in a mixture of salts we may have more than one state of equilibrium possible. And when a salt, like alum, happens to be dissolved in such a way as to decompose it, the constituents are not necessarily reunited by subsequent mixing. Many practices in the chemical arts, which seem empirical, have their foundation possibly in facts of this kind.

8. *Diffusion of one Salt into the Solution of another Salt.*

It was curious and peculiarly important, in reference to the relation of liquid to gaseous diffusion, to find whether one salt A would diffuse into water already charged with an equal or greater quantity of another salt B, as a gas *a* freely diffuses into the space already occupied by another gas *b*; the gas *b* in return diffusing at the same time into the space occupied by *a*. Or whether, on the contrary, the diffusion of the salt A is resisted by B. The latter result would indicate a neutralization of the water's attraction, and a kind of equivalency or equality of power and exchangeability of different salts, in respect of that effect, which would divide entirely the phenomena of liquid from those of gaseous diffusion.

(1.) A solution of 4 parts of carbonate of soda to 100 water, of density 1.0406, was placed in the six-ounce diffusion phial of 1.175 inch aperture, and allowed to communicate with 24 ounces of water.

Two similar diffusion phials, equally charged, were immersed in 24 ounces of a solution of 4 parts of chloride of sodium to 100 water, having the density 1.0282. The diffusion proceeded for eight days, in all cases, at 64°. The proportion of carbonate of soda found without in the water-jar

afterwards, was ascertained by an alkalimetical process, the neutralization being effected at the boiling-point. The following are the results:—

Experiment I. Diffusion product	} 9.06 grs. of carbonate of soda.
into water	
Experiment II. Diffusion product	} 8.82 grs. of carbonate of soda.
into solution of chloride of sodium	
Experiment III. Diffusion product	} 9.10 grs. of carbonate of soda.
into solution of chloride of sodium	

It thus appears that 4 per cent. of chloride of sodium present in the water atmosphere of the jar has no sensible effect in retarding the diffusion into it, from the solution cell, of carbonate of soda from a solution containing also 4 per cent. of the latter.

(2.) The experiment was varied by allowing the solution of carbonate of soda to diffuse into a solution of sulphate of soda, a salt more similar to the former in solubility and composition. The solution of the latter, containing 4 per cent., was of density 1.0352. The temperature and period of diffusion were the same as before:—

Experiment IV. Diffusion product	} 7.84 grs. of carbonate of soda.
into solution of sulphate of soda	
Experiment V. Diffusion product	} 7.82 grs. of carbonate of soda.
into solution of sulphate of soda	

Here we find a small reduction in the quantity of carbonate of soda diffused, amounting to one-eighth of the whole. The sulphate of soda has therefore exercised a positive interference in checking the diffusion of the carbonate to that extent. So small and disproportionate an effect however is scarcely sufficient to establish the existence of a mutual elasticity and resistance between these two salts.

Still it might be said, may not the diffusion of one salt be resisted by another salt which is strictly isomorphous with the first?

(3.) A solution of 4 parts of nitrate of potash to 100 of water, of density 1.0241, placed in the solution phial, was allowed to communicate with water containing 4 per cent. of nitrate of ammonia in the water-jar, which last solution was of density 1.0136; with all other circumstances as before. With one solution phial having the usual aperture, 1.175 inch, the diffusion product was 15.32 grs. of nitrate of potash. With a second phial, having a larger aperture of 1.190 inch, the diffusion product was 18.03 grs. of nitrate of potash. No comparative experiment, on the diffusion of nitrate of potash into water, was made at the same time. But nitrate of ammonia, which appeared before to coincide in diffusibility with

nitrate of potash, gave on a former occasion, in similar circumstances, and at $64^{\circ}9$, nearly the same temperature, a diffusion product of 15.80 grs. The quantity of nitrate of potash (15.32 grs.) which diffused into the solution of nitrate of ammonia approaches so closely to the number quoted, that we may safely conclude that the diffusion of nitrate of potash is not sensibly resisted by nitrate of ammonia, although these two salts are closely isomorphous. They are still therefore inelastic to each other, like two different gases.

These experiments have been made upon dilute solutions, and it is not at all impossible that the result may be greatly modified in concentrated solutions of the same salts, or when the solutions approach to saturation. But there is reason to apprehend that the phenomena of liquid diffusion are exhibited in the simplest form by dilute solutions, and that concentration of the dissolved salt, like compression of a gas, is attended often with a departure from the normal character.

On approaching the degree of pressure which occasions the liquefaction of a gas, an attraction appears to be brought into play, which impairs the elasticity of the gas; so on approaching the point of saturation of a salt, an attraction of the salt molecules for each other, tending to produce crystallization, comes into action, which will interfere with and diminish that elasticity or dispersive tendency of the dissolved salt which occasions its diffusion.

We are perhaps justified in extending the analogy a step further between the characters of a gas near its point of liquefaction and the conditions which we may assign to solutions. The theoretical density of a liquefiable gas may be completely disguised under great pressure. Thus, under a reduction by pressure of 20 volumes into 1, while the elasticity of air is 19.72 atmospheres, that of carbonic acid is only 16.70 atmospheres, and the deviation from their normal densities is in the inverse proportion. Of salts in solution the densities may be affected by similar causes, so that although different salts in solution really admit of certain normal relations in density, these relations may be concealed and not directly observable.

The analogy of liquid diffusion to gaseous diffusion and vaporization is borne out in every character of the former which has been examined. Mixed salts appear to diffuse independently of each other, like mixed gases, and into a water atmosphere already charged with another salt as into pure water. Salts also are unequally diffusible, like the gases, and separations, both mechanical and chemical (decompositions), are produced by liquid as well as by gaseous diffusion. But it still remains to be found whether the diffusibilities of different salts are in any fixed proportion to each other, as simple

numerical relations are known to prevail in the diffusion velocities of the gases, from which their densities are deducible.

It was desirable to make numerous simultaneous observations on the salts compared, in order to secure uniformity of conditions, particularly of temperature. The means of greatly multiplying the experiments were obtained by having the solution phial cast in a mould, so that any number of solution cells could be procured of the same form and dimensions.

The phials were of the form represented (fig. 3), holding about 4 ounces, or more nearly 2080 grs. of water to the base of the neck, and the mouths of all were ground down, so as to give the phial a uniform height of 3·8 inches. The mouth or neck was also ground to fit a gauge-stopper of wood, which was 0·5 inch deep and slightly conical, being 1·24 inch in diameter on the upper, and 1·20 inch on the lower surface. These are therefore the dimensions of the diffusion aperture of the new solution cells. A little contrivance to be used in filling the phials to a constant distance of half an inch from the surface of the lip, proved useful. It was a narrow slip of brass plate, having a descending pin of exactly half an inch in length fixed on one side of it (fig. 4).



Fig. 3.

This being laid across the mouth of the phial with the pin downwards in the neck, the solution was poured into the phial till it reached the point of the pin. The brass plate and pin being removed, the neck was then filled up



Fig. 4.

with distilled water, with the aid of the little float as before described. The water-jar, in which the solution phial stood, was filled up with water also as formerly, so as to cover the phial entirely to the depth of 1 inch. This water atmosphere amounted to 8750 grs., or about 20 ounces. A glass plate was placed upon the mouth of the water-jar itself to prevent evaporation. Sometimes 80 or 100 diffusion cells were put in action at the same time. The period of diffusion chosen was now always exactly seven days, unless otherwise mentioned.

II. DIFFUSION OF SALTS OF POTASH AND AMMONIA.

Solutions were prepared of the various salts, in a pure state, in certain fixed proportions, namely, 2, 4, $6\frac{2}{3}$ and 10 parts of salt to 100 parts of water by weight. The density of these solutions was observed by the weighing-bottle, at 60°. The solutions were frequently diffused at two different tempera-

tures; one, the temperature of the atmosphere, which was fortunately remarkably constant during most of the experiments to be recorded at present, and the other, a lower temperature, obtained in a close box of large dimensions, containing masses of ice. The results at the artificial temperature were obviously less accurate than those of the natural temperature, but have still considerable value. Three experiments were generally made upon the diffusion of each solution at the higher, with two experiments at the lower temperature.

(1.) The carbonate and sulphate of potash and sulphate of ammonia were first diffused during a period of seven days, of which the temperatures observed by a thermometer placed near the water-jars were $64^{\circ}\cdot5$, 65° , $63^{\circ}\cdot5$, 63° , 63° , $63^{\circ}\cdot5$, 65° and 66° ; mean temperature $64^{\circ}\cdot2$.

Table VII.--Diffusion of Carbonate of Potash, Sulphate of Potash and Sulphate of Ammonia.

Parts of anhydrous salt to 100 water.	Density of solution at 60° .	At $64^{\circ}\cdot2$.		At $37^{\circ}\cdot6$.	
		Experiments.	Mean.	Experiments.	Mean.
Carbonate of potash.					
2	1·0178	5·36 5·55	5·45	3·80 3·91	3·85
4	1·0347	10·39 10·11	10·25	6·99 7·19	7·09
$6\frac{2}{3}$	1·0572	16·50 16·46 17·05	16·67	11·42 11·08	11·25
10	1·0824	24·42 24·94 24·70	24·69		
Sulphate of potash.					
2	1·0155	5·62 5·42	5·52	3·93 3·98	3·95
4	1·0318	10·49 10·65	10·57	7·50 7·31	7·40
$6\frac{2}{3}$	1·0512	17·07 16·89 17·54	17·17	11·62 11·71	11·66
10	1·0742	23·40 23·59 23·88	23·62		
Sulphate of ammonia, NH^4O , SO^5 .					
2	1·0117	5·71 5·45	5·58	3·73 3·79	3·76
4	1·0229	10·72 10·30	10·51	7·54 7·86	7·70
$6\frac{2}{3}$	1·0369	17·28 16·28 16·80	16·79	10·94 10·98	10·96
10	1·0529	21·86 22·49 22·25	22·20		

The diffusion product was obtained by evaporating the water of each jar separately as before, and the result is expressed in grains.

It will be observed at once, on comparing the means of the experiments, that the three salts under consideration are remarkably similar in their diffusion, particularly with the smaller proportions of salt. Thus the mean diffusion of the 2, 4, $6\frac{2}{3}$ and 10 parts of the salts is as follows:—

Diffusion at $64^{\circ}2$.

	2.	4.	$6\frac{2}{3}$.	10.
Carbonate of potash ...	5.45	10.25	16.67	24.69
Sulphate of potash	5.52	10.57	17.17	23.62
Sulphate of ammonia...	5.58	10.51	16.79	22.20

Diffusion at $37^{\circ}6$.

	2.	4.	$6\frac{2}{3}$.
Carbonate of potash	3.85	7.09	11.25
Sulphate of potash	3.95	7.40	11.66
Sulphate of ammonia.....	3.76	7.70	10.96

The proportions diffused are sensibly equal, of the different salts, at the higher temperature, with the exception of the largest proportion of salt, 10 per cent., when a certain divergence occurs. This last fact is consistent with our expectations, that the diffusion of salts would prove most highly normal in dilute solutions. Some of the irregularities at the lower temperature are evidently of an accidental kind.

(2.) The neutral chromate and acetate of potash were diffused at a temperature ranging from 63° to 65° , or at a mean temperature of $64^{\circ}1$, which almost coincides with the higher temperature of the last experiments.

Table VIII.—Diffusion of Chromate of Potash and Acetate of Potash, at 64°1.

Parts of anhydrous salt to 100 water.	Density of solution at 60°.	Experiments.	Mean.
Chromate of potash.			
2	1·0158	5·79 5·66 5·86	5·77
4	1·0313	11·10 11·35 11·13	
6 $\frac{2}{3}$	1·0512	17·76 17·72 17·32	11·19
10	1·0750	24·49 24·92 24·85	17·60
			24·75
Acetate of potash.			
2	1·0095	5·93 5·75 5·88	5·85
4	1·0184	10·55 10·56 10·98	
6 $\frac{2}{3}$	1·0306	16·53 16·06 16·84	10·70
10	1·0447	24·27 24·82 25·46	16·48
			24·85

We have the same close correspondence in the diffusion products of these two salts as in the preceding group, and here the correspondence extends to the 10 per cent. solution.

Diffusion at 64°1.

	2.	4.	6 $\frac{2}{3}$.	10.
Chromate of potash ...	5·77	11·19	17·60	24·75
Acetate of potash	5·85	10·70	16·48	24·85

The 10 per cent. solution of these two salts also agrees with the same solution of carbonate of potash, which was 24·69 grs. Nor do the lower proportions diverge greatly from the preceding group of salts.

(3.) Another pair of salts were simultaneously diffused, but with an accidental difference of $0^{\circ}\cdot 4$ of temperature.

Table IX.—Diffusion of Bicarbonate of Potash, $\text{KO}, \text{CO}^2 + \text{HO}, \text{CO}^2$, at $64^{\circ}\cdot 1$, and Bichromate of Potash, $\text{KO}, 2\text{CrO}^3$, at $64^{\circ}\cdot 5$.

Parts of anhydrous salt to 100 water.	Density of solution at 60° .	At $64^{\circ}\cdot 1$ and $64^{\circ}\cdot 5$.	
		Experiments.	Mean.
Bicarbonate of potash. 2	1·0129	5·74 5·77 5·91	5·81
4	1·0252	10·75 11·16 11·13	
Bichromate of potash. 2	1·0139	5·64 5·73 5·59	5·65
4	1·0273	11·55 11·54 11·39	

Here again the two salts approach closely in diffusion, and also correspond well with the two preceding series.

Mean Diffusion at $64^{\circ}\cdot 1$ and $64^{\circ}\cdot 5$.

	2.	4.
Bicarbonate of potash	5·81	11·01
Bichromate of potash	5·65	11·49

It is singular to find that salts differing so much in constitution and atomic weight as the chromate and bichromate of potash, may be confounded in diffusibility. The diffusion products of these two salts are, for the 2 per cent. solutions, 5·77 and 5·65 grs., and for the 4 per cent. solution, 11·19 and 11·49 grs. The bicarbonate of potash also exhibits a considerable analogy to the carbonate, but resembles still more closely the acetate. It is thus obvious that equality, or similarity, of diffusion is not confined to the isomorphous groups of salts.

(4.) The nitrates of potash and ammonia have already appeared to be equidiffusive at two different temperatures. They were diffused again in the same proportions as the last salts, at a temperature varying from 63° to $67^{\circ}\cdot 5$.

Table X.—Diffusion of Nitrate of Potash and Nitrate of Ammonia at 65°·9.

Parts of anhydrous salt to 100 water.	Density of solution at 60°.	Experiments.	Mean.
Nitrate of potash.			
2	1·0123	7·34 7·58 7·49	7·47
4	1·0243	13·66 14·24 14·02	13·97
6 $\frac{2}{3}$	1·0393	22·11 22·94 22·05	22·37
10	1·0581	32·06 32·90 32·50	32·49
Nitrate of ammonia NH ⁴ O, NO ⁵ .			
2	1·0080	7·85 7·71 7·64	7·73
4	1·0154	14·20 14·79 14·45	14·48
6 $\frac{2}{3}$	1·0256	23·66 23·35 22·22	22·74
10	1·0375	34·94 33·49 34·23	34·22

The solution of nitrate of ammonia of the water-jars was evaporated carefully at a temperature not exceeding 120° F., to prevent loss of the salt by sublimation or decomposition.

Diffusion at 65°·9.

	2.	4.	6 $\frac{2}{3}$.	10.
Nitrate of potash	7·47	13·97	22·37	32·49
Nitrate of ammonia	7·73	14·48	22·74	34·22

Although these salts correspond closely, it is probable that neither the diffusion of these nor the diffusion of any others is absolutely identical. The nitrate of ammonia appears to possess a slight superiority in diffusion over the nitrate of potash, which increases with the large proportions of salt in solution. They are both considerably more diffusible than the seven preceding salts.

(5.) A second pair of isomorphous salts were compared, the chlorides of potassium and ammonium.

Table XI.—Diffusion of Chloride of Potassium and Chloride of Ammonium.

Parts of anhydrous salt to 100 water.	Density of solution at 60°.	At 66°·2.		At 64°·7	
		Experiments.	Mean.	Experiments.	Mean.
Chloride of potassium.					
2	1·0127	7·83 7·72 7·55	7·70	8·03 7·89	7·96
4	1·0248	15·22 15·59 15·07	15·29	15·21 14·82	15·01
6 $\frac{2}{3}$	1·0401	24·88 24·64 25·09	24·87	24·83 24·62	24·72
10	1·0592	36·23 37·63	36·93		
Chloride of ammonium.					
2	1·0061	7·10 8·52	7·81	7·10 7·24	7·17
4	1·0118	14·55 14·64	14·60	13·91 14·91	14·41
6 $\frac{2}{3}$	1·0190	24·30	24·30	24·12 24·13	24·12
10	1·0272	36·53	36·53		

These two salts agree well in diffusibility, and are also evidently related to the preceding nitrates. The quantity of chloride of ammonium diffused was determined by evaporation, which is troublesome and may lead to small errors, from the volatility and efflorescent tendency of this salt. It would be easier and more accurate to determine this and other chlorides by the use of a normal solution of nitrate of silver, and so avoid evaporation.

Diffusion at 66°·2.

	2.	4.	6 $\frac{2}{3}$.	10.
Chloride of potassium	7·70	15·29	24·87	36·93
Chloride of ammonium	7·81	14·60	24·30	36·53

The quantities diffused of these two chlorides are more closely in proportion to the strength of the original solution, than with any of the preceding salts of potash. Thus the quantities diffused from the 2 and 10 per cent. solutions of chloride of potassium are 7·70 and 36·93 grs., which are as 2 to 9·6, which is nearly as 2 to 10. Chloride of sodium was observed before to be nearly uniform in this respect; but other salts appear to lose considerably in diffusibility with the higher

proportions of salt. It is possibly a consequence of the crystallizing attraction, to which reference was lately made, coming into action in strong solutions and resisting diffusion

(6.) The diffusion of chlorate of potash was observed at a temperature ranging from 63° to 65°, of which the mean was 64°·1.

Table XII.—Diffusion of Chlorate of Potash.

Parts of salt to 100 water.	Density of solution at 60°.	At 64°·1	
		Experiments.	Mean.
2	1·0129	6·97 7·54 7·16	7·22
4	1·0246	13·03 13·64 13·27	13·31
6·5 (saturated solution).	1·0395	21·30 20·29 20·76	20·78

The solutions of chlorate of potash must be evaporated and the residuary salt dried at a temperature not exceeding 212°, otherwise a very sensible quantity of chloride of potassium may be formed. The chlorate appears to be sensibly inferior in diffusibility to the nitrate of potash. From the 4 per cent. solution of the chlorate we have a diffusion product of 13·27 grs., and from the corresponding solution of the nitrate 13·97 grs.; but the latter was obtained at a temperature 1°·8 higher than the former. It remains a question whether chlorate of potash does not really belong to the nitre group of salts, but has its diffusion interfered with by some secondary agency, such as its sparing solubility and consequent nearer approach to the saturating proportion.

It is certainly true that the uniformity of diffusion generally increases with the dilution of the solutions. This appears on comparing the diffusion of the 4 per cent. solution of what may be called the sulphate of potash group, with the diffusions of the 2 per cent. solutions of the same salts.

Diffusion of Salts of the Sulphate of Potash Class.

	4.	2.
Carbonate of potash	10·27	5·45
Sulphate of potash	10·57	5·52
Sulphate of ammonia.....	10·51	5·58
Acetate of potash	10·70	5·85
Bicarbonate of potash	11·01	5·81
Chromate of potash	11·19	5·77
Bichromate of potash	11·49	5·65

Thus while the 4 per cent. solutions range from 10·27 to 11·49 grs., or from 100 to 111·8, the 2 per cent. solutions range from 5·45 grs. to 5·85 grs., or from 100 to 107·3.

As it appeared to be in dilute solutions that the greatest uniformity of diffusion is to be expected, a series of experiments was instituted upon the preceding salts, with the addition of acetate of potash, which appeared to belong to the same class, the solution employed being that of 1 salt to 100 water. The experiments were made in a vault, of which the temperature was nearly uniform, falling in a gradual manner from 59° to 58°, with a mean of 58°·5 during the period of seven days which the diffusion lasted. Eight phials of each salt were diffused, and the liquids of four water-jars evaporated together.

Carbonate of potash gave 10·42 and 10·59 grs. of salt diffused: mean 10·51 grs., or 2·63 grs. for one cell.

Sulphate of potash gave 10·72 and 10·78 grs. of salt diffused: mean 10·75 grs., or 2·69 grs. for one cell.

Acetate of potash, its diffusion product being treated with an excess of hydrochloric acid, gave 8·30 and 8·04 grs. of chloride of potassium, equivalent to 10·91 and 10·57 grs. of acetate of potash; mean 10·74 grs. of acetate of potash, or 2·68 grs. for one cell. The diffusion of these three salts is therefore remarkably similar:—

Diffusion of 1 per cent. solutions at 58°·5.

Carbonate of potash	2·63 grs.
Sulphate of potash	2·69 grs.
Acetate of potash	2·68 grs.

The 1 per cent. solution of neutral or yellow chromate of potash in good crystals gave 11·28 and 11·35 grs.; mean 11·31 grs., or 2·83 grs. for each cell. It was remarked of the diffused chromate in this experiment, that it contained a sensible quantity of green oxide of chromium. The diffusion of a salt appears indeed to try its tendencies to decomposition very severely.

The bicarbonate of potash gave 8·83 and 8·35 grs. of chloride of potassium, the diffusion product being neutralized with hydrochloric acid; equivalent to 11·25 and 11·21 grs. of bicarbonate of potash; mean 11·23 grs., or 2·81 grs. for one cell.

The bichromate of potash gave 11·54 and 11·49 grs. of salt diffused; mean 11·51 grs., or 2·88 grs. for one cell. These last three salts give all a larger diffusion product than the preceding three, while they agree well together. It is doubtful whether this excess in their diffusion is occasioned by a partial decomposition in the act of diffusion, which might be of

such a kind as to increase the real or apparent diffusion in every one of them, or whether it is a peculiar character of this little group, to which the ferricyanide of potassium, it will be afterwards seen, falls to be added, while the ferrocyanide appears to belong to the other group:—

Diffusion of 1 per cent. solutions at 58°·5.

Chromate of potash	2·83 grs.
Bicarbonate of potash	2·81 grs.
Bichromate of potash	2·88 grs.

The divergence from each other of two salts so closely isomorphous as sulphate and chromate of potash, in the proportion of 100 to 105·2, is certainly remarkable, unless due to a slight decomposition of the latter.

(7.) *Ferrocyanide and Ferricyanide of Potassium.*

Of these two salts the 1 per cent. solution only was diffused. The time of diffusion was seven days, as usual; the mean temperature 54°·5. In evaporating the liquid of the water-jars, both salts were partially decomposed, so that it became necessary to estimate the diffusion product by a determination of the potash. Eight cells were employed for one salt and six for the other, and the liquids of the water-jars evaporated two together.

The diffusion product of ferrocyanide of potassium (anhydrous) was 5·02, 5·22, 5·02 and 5·20 grs.; mean 5·12 grs., or for one cell 2·56 grs.

The diffusion product of ferricyanide of potassium was 5·54, 5·64 and 5·36 grs.; mean 5·51 grs., or for one cell 2·75 grs.

Three cells of a similar solution of sulphate of potash which were diffused for seven days at a mean temperature 1° lower, or of 53°·5, gave 2·56, 2·53 and 2·62 grs.; mean for one cell 2·57 grs., a number which almost coincides with that of the ferrocyanide of potassium (2·56 grs.). The ferricyanide of potassium, on the other hand, is sensibly more diffusive, as 107·6 to 100, and appears to rank with the bicarbonate and bichromate of potash. The ferricyanide of potassium, again, is a salt which probably undergoes a slight decomposition in diffusion like those salts mentioned:—

Diffusion of 1 per cent. solutions.

Sulphate of potash	2·57 grs. at 53°·5.
Ferrocyanide of potassium . .	2·56 grs. at 54°·5.
Ferricyanide of potassium . .	2·75 grs. at 54°·5.

The salts of the nitre class may also be compared in the *Phil. Mag.* S. 3. Vol. 37. No. 250. Oct. 1850. T

same manner, and I shall now add a third series of results obtained from the diffusion of 1 per cent. solutions of the same salts. The temperature of diffusion of this new series was $64^{\circ}5$. Six phials of each salt were diffused, and they were evaporated afterwards two and two. This double diffusion product, however, is divided by 2 in the table.

Diffusion of Salts of the Nitre Class.

	4.	2.	1.
Nitrate of potash	13.97	7.47	3.72
Nitrate of ammonia	14.48	7.73	3.75
Chloride of potassium	15.01	7.70	3.88
Chloride of ammonium	14.41	7.81	3.89
Chlorate of potash.....	13.31	7.22	3.66
Mean	14.23	7.58	3.78

It is interesting to observe how the chlorate of potash rises in the lower proportions and approaches to the normal rate of its class. The diffusion products of all the salts are obviously more uniform for the two than for the 4 per cent. solutions, and again more uniform for the 1 than for the 2 per cent. solutions. The extremes in the 1 per cent. solutions are 3.66 grs. chlorate of potash, and 3.89 grs. chloride of ammonium, which are as 1 to 1.0628. We have here an approach to equality in diffusion, which appears to be as close as the experimental determinations are of the specific heat of different bodies belonging to one class. The numbers for the specific heat of equivalents of the metallic elements are known to vary as 38 to 42.

The salts of potash thus appear to fall into two groups of very similar if not equal diffusibility. What is the relation between these groups?

The diffusion of 4 per cent. solutions of carbonate and nitrate of potash was repeated at a temperature rising gradually from 63° to 65° during the seven days of the experiment, with a mean of $64^{\circ}1$. The diffusion products of the carbonate were 10.31, 10.05 and 10.44 grs. in three cells; mean 10.27 grs. Of the nitrate, 13.98, 13.86 and 13.60 grs.; mean 13.81 grs. We have thus a diffusion in equal times of—

Carbonate of potash . . .	10.27	1
Nitrate of potash . . .	13.81	1.3447

These experiments are almost identical with the former results, 10.25 carbonate of potash, and 13.97 nitrate of potash.

But the numbers thus obtained cannot be fairly compared, owing to the diminishing progression in which the diffusion of

a salt takes place. Thus when the diffusion of nitrate of potash was interrupted every two days, as in a former experiment with chloride of sodium, the progress of the diffusion for eight days was found to be as follows in a 4 per cent. solution, with a mean temperature of 66°.

Nitrate of Potash.

Diffused in first two days . .	4.54 grs.
Diffused in second two days . .	4.13 grs.
Diffused in third two days . .	4.06 grs.
Diffused in fourth two days . .	3.18 grs.
	15.91

The absence of uniformity in this progression is no doubt chiefly due to the want of geometrical regularity in the form of the neck and shoulder of the solution phial. A plain cylinder, as the solution cell, might give a more uniform progression, but would increase greatly the difficulties of manipulation.

The diffusion of carbonate of potash will no doubt follow a diminishing progression also; but there is this difference, that the latter salt will not advance so far in its progression, owing to its smaller diffusibility, in the seven days of the experiment, as the more diffusible nitrate does. The diffusion of the carbonate will thus be given in excess, and as it is the smaller diffusion, the difference of the diffusion of the two salts will not be fully brought out.

The only way in which the comparison of the two salts can be made with perfect fairness, is to allow the diffusion of the slower salt to proceed for a longer time, till in fact the quantity diffused is the same for this as for the other salt, and the same point in the progression has therefore been obtained in both; and to note the time required. The problem takes the form of determining the times of equal diffusion of the two salts. This procedure is the more necessary from the inapplicability of calculation to the diffusion progression.

Further, allowing the Times of Equal Diffusion to be found, it is not to be expected that they will present a simple relation. Recurring to the analogy of gaseous diffusion, the times in which equal volumes or equal weights of two gases diffuse are as the square roots of the densities of the gases. The times, for instance, in which equal quantities of oxygen and hydrogen escape out of a vessel into the air, in similar circumstances, are as 4 to 1; the densities of these two gases as 16 to 1. Or, the times of equal diffusion of oxygen and protocarburetted hydrogen are as 1.4142 to 1, that is as the

square root of 2 to the square root of 1; the densities of these gases being 16 and 8, which are as 2 to 1. The densities are the squares of the equal-diffusion times. It is not therefore the times themselves of equal diffusion of two salts, but the squares of those times which are likely to exhibit a simple relation.

(1.) While the 4 per cent. solution of nitrate of potash was diffused as usual for seven days, the corresponding solution of carbonate of potash was now allowed to diffuse for 9.90 days; times which are as 1 to 1.4142, or as 1 to the square root of 2.

The results were as follows: diffused of—

Nitrate of potash at $64^{\circ}1$, in seven days, 13.81 grs. 100
Carbonate of potash at $64^{\circ}3$, in 9.9 days, 13.92 grs. 100.8

The three experiments on the nitrate of potash, of which 13.81 grs. is the mean, were 13.98, 13.86 and 13.60 grs., as already detailed. The three experiments on the carbonate were 14.00, 13.97 and 13.78 grs. The difference in the means of the two salts is only 0.11 gr. The results appear to be as near to equality as could be reasonably expected from the method of experimenting. Seven and 9.90 may therefore be considered as the times of equal diffusion indicated for nitrate and carbonate of potash. The times of equal diffusion, or the diffusibilities of nitrate and carbonate of potash, would appear therefore to be in the proportion of the square root of 1 to the square root of 2.

The explanation of such a relation suggested by gaseous diffusion has been anticipated. It is that the two salts have different densities in solution, that of nitrate of potash being 1, and that of carbonate of potash 2. We are thus led to ascribe, what may be called Solution Densities, to the salts. The two salts in question are related exactly like protocarburetted hydrogen gas, of density 1, to oxygen gas of density 2. The parallel would be completed by supposing that the single volume of oxygen to be diffused was previously mixed with 100 volumes of air (or any other diluting gas), while the 2 volumes of protocarburetted hydrogen were also diluted with 100 volumes of air; the diluting air here representing the water in which the salts to be diffused are dissolved in the solution cell. The time in which a certain quantity of protocarburetted hydrogen would come out from a vessel containing 1 per cent. of that gas being 1 (the square root of density 1), the time in which an equal quantity of oxygen would diffuse out from a similar vessel containing 1 per cent. also would be 1.4142 (the square root of density 2).

(2.) A solution of 4 parts of sulphate of potash in 100 water was diffused simultaneously with the last solution of carbonate of potash, and therefore in similar circumstances. The diffusion products of three experiments were 14.46, 14.21 and 14.53 grs.; mean 14.40 grs. This is in the proportion of 104.27 sulphate of potash to 100 nitrate of potash; so that the approximation to equality of diffusion with nitrate of potash, in the selected times, is not so close for the sulphate as for the carbonate of potash.

(3.) The diffusion was repeated of 2 per cent. solutions of the nitrate and carbonate of potash at a lower temperature by about 10° . The temperature of the solutions was rather unsteady; ranging from 56° to $52^{\circ}25$ for the first period of seven days, from 56° to $50^{\circ}5$ for the period of 9.90 days, and from 55° to $50^{\circ}5$ for a second period of seven days; the external atmospheric temperature having fallen during the same period more than 20 degrees. Six phials of each solution were diffused and evaporated two together; so that the results are all double quantities.

At a mean temperature of $54^{\circ}3$, the nitrate of potash gave in seven days 12.60 and 12.13 grs.; mean 12.36 grs.

Again, at a mean temperature of $52^{\circ}4$, the nitrate of potash gave in seven days 11.85, 12.40 and 11.95 grs.; mean 12.06 grs.

The carbonate of potash gave in 9.90 days, with a mean temperature of $53^{\circ}4$, 12.69, 12.40 and 12.12 grs.; mean 12.40 grs.

The general results are—

Nitrate of potash, in seven days, at $54^{\circ}3$	. 12.36 grs.
Carbonate of potash, in 9.9 days, at $53^{\circ}4$	. 12.40 grs.
Nitrate of potash, in seven days, at $52^{\circ}4$	. 12.06 grs.

As the first nitrate is $0^{\circ}9$ above the carbonate and the second nitrate 1° below it, we may take the mean of the two nitrates as corresponding to the temperature of the carbonate. We thus finally obtain, diffused at $53^{\circ}4$, of—

Nitrate of potash in seven days, 12.22 grs.	. 100
Carbonate of potash in 9.9 days, 12.40 grs.	. 101.47

The difference in the amount of the diffusion of the two salts in these times is only 0.18 gr., or $1\frac{1}{2}$ per cent.

These last experiments may be held therefore as tending to the same conclusion as the former series, although the circumstances were more than usually unfavourable to their success. To find whether the same relation existed between the salts through a considerable range of temperature, an opportunity

was taken during cold weather to repeat the experiments at a low temperature.

(4.) Solutions of 1 salt in 100 water were diffused from eight solution cells, for each salt. The times were increased, but the same ratio of 1 to 1.4142 was preserved between them. The liquids of the cells were found to retain a temperature ranging slowly between 41° and $38^{\circ}8$ during the whole period of the observations. Sulphate of potash was substituted for the carbonate, as of these two equi-diffusive salts the former had been found to be least in accordance with nitrate of potash, in the 4 per cent. solutions, and appeared therefore to afford the severest test of the relation.

For nitrate of potash, at a mean temperature of $39^{\circ}7$, during nine days, the diffusion product of two cells together was 6.97, 6.93, 6.77 and 6.64 grs.; mean 6.83 grs. for two cells.

For sulphate of potash, at the same mean temperature of $39^{\circ}7$, during 12.728 days (twelve days, seventeen hours, twenty-eight minutes), the diffusion product of two cells together was 7.05, 6.93, 7.28 and 6.90 grs.; mean 7.04 grs. for two cells.

The general results are—

Nitrate of potash in nine days at $39^{\circ}7$	6.83 grs.	100
Sulphate of potash in 12.728 days at $39^{\circ}7$	7.04 grs.	103.07

(5.) Solutions of 2 salt in 100 water were diffused simultaneously with the preceding experiments, and in precisely the same conditions of time and temperature.

The diffusion product of nitrate of potash during nine days, at a mean temperature of $39^{\circ}7$, was 7.03, 6.63, 6.83 and 6.83 grs. for one cell; mean 6.83 grs. for one cell, or the same number as for two cells with the 1 per cent. solution.

The diffusion product of sulphate of potash during 12.728 days was 6.84, and 6.80; mean 6.82 grs. for one cell. These experiments almost coincide with the number for nitrate of potash.

Nitrate of potash, 6.83 grs.	. . .	100
Sulphate of potash, 6.82 grs.	. .	99.85

(6.) The existence of the relation in question was also severely tested in another manner. Preserving the ratio in the times of diffusion for the two salts, the actual times were varied in duration, in three series of experiments, as 1, 2 and 3. The experiments were made in the vault, with a uniformity of temperature favourable to accuracy of observation. Eight cells of the 1 per cent. solution of each salt were always diffused at the same time.

(a.) Nitrate of potash diffused for 3.5 days, at $47^{\circ}2$, gave

for two cells, 3·55, 3·63, 3·33 and 3·51 grs.; mean for two cells, 3·50 grs.

Sulphate of potash diffused for 4·95 days, at $47^{\circ}3$, gave for two cells, 3·54, 3·31, 3·51 and 3·63 grs.; mean for two cells, 3·50 grs., or exactly the same as for nitrate of potash above.

(b.) Nitrate of potash diffused for seven days, at $48^{\circ}6$, gave 6·1, 6·2, 5·9 and 5·92 grs.; mean for two cells, 6·04 grs.

Sulphate of potash diffused for 9·9 days, at $49^{\circ}1$, gave 6·13, 5·92, 6·18 and 6·59 grs.; mean 6·20 grs., or, excluding the last experiment, 6·08 grs.

Chromate of potash diffused also for 9·9 days, at $49^{\circ}1$, gave 6·19, 6·18, 6·40 and 6·38 grs.; mean for two cells, 6·29 grs. The diffused chromate presented no appearance of decomposition on this occasion.

(c.) Nitrate of potash diffused for 10·5 days, at 48° , gave 8·36, 8·95, 8·82 and 8·84 grs.; mean for two cells, 8·74 grs.

Sulphate of potash diffused for 14·85 days, at $48^{\circ}6$, gave 8·99, 8·94, 8·66 and 8·56 grs.; mean for two cells, 8·79 grs.

The mean results for the three different sets of periods of diffusion are as follows:—

3·5 and 4·95 days	{	Nitrate of potash, at $47^{\circ}2$, 3·50 grs.	100
		Sulphate of potash, at $47^{\circ}3$, 3·50 grs.	100
7 and 9·9 days	{	Nitrate of potash, at $48^{\circ}6$, 6·04 grs.	100
		Sulphate of potash, at $49^{\circ}1$, 6·20 grs.	102·65
10·5 and 14·85 days	{	Chromate of potash, at $49^{\circ}1$, 6·29 grs.	104·14
		Nitrate of potash, at 48° , 8·74 grs.	100
		Sulphate of potash, at $48^{\circ}6$, 8·79 grs.	100·57

The concurring evidence of these three series of experiments appears to be quite decisive in favour of the assumed relation of 1 to 1·4142, between the times of equal diffusion for the nitrate and sulphate of potash, and consequently of the times for the two classes of potash salts, of which the salts named are types. The same experiments are also valuable as proving the similarity of the progression of diffusion, in two salts of unequal diffusibility. I shall return again to the relation between nitrates and sulphates, under the salts of soda.

(8.) *Hydrate of Potash.*

(1.) Eight cells of the 1 per cent. solution of pure fused hydrate of potash were diffused for seven days in the vault, with a temperature ranging only from 59° to 58° , of which the mean was $58^{\circ}6$. The product of four cells evaporated together was 17·57 grs. of hydrate of potash, and of the other four cells 17·19 grs.; mean 17·38 grs., or 4·345 grs. for one cell. The hydrate of potash was estimated from the chloride

of potassium which it gave when saturated with hydrochloric acid. The diffusion product of sulphate of potash for seven days, at $58^{\circ}5$, or almost the same temperature, was 10.75 grs. for the four cells, as already stated, and consequently 2.64 grs. for one cell. It thus appears that the hydrate of potash is greatly more diffusive than the sulphate of potash in the same period of seven days, namely, as 4.345 to 2.64. Such a result indeed is not inconsistent with the times of equal diffusion of these two substances, differing as much as 1 to 2.

(2.) Of pure fused hydrate of potash, a 1 per cent. solution was diffused from four cells for 4.95 days at a mean temperature of $53^{\circ}7$, against a 1 per cent. solution of nitrate of potash in six cells, for seven days, at a mean temperature $0^{\circ}1$ lower, or of $53^{\circ}6$. The hydrate of potash which diffused, is calculated as before from the chloride of potassium which it gave, when neutralized by hydrochloric acid. Hydrate of potash diffused from two cells 5.97 and 6.28 grs.; mean 6.12 grs., or 3.06 grs. for a single cell.

Nitrate of potash diffused from two cells 6.22, 6.54 and 5.93 grs.; mean 6.23 grs., or 3.11 grs. for a single cell. The diffusion of nitrate of potash being 100, that of the hydrate of potash is 98.2, numbers which are sufficiently in accordance. But the times were as 1 to 1.4142, and their squares as 1 to 2. So far then as this series of experiments on hydrate of potash entitles us to conclude, we appear to have for the salts of potash a close approximation to the following simple series of squares of equal diffusion times:—

Squares of Times of Equal Diffusion, or Solution Densities.

Hydrate of potash	. . .	1
Nitrate of potash	. . .	2
Sulphate of potash	. . .	4

(3.) The hydrate of potash was also diffused at the lower temperature, $39^{\circ}7$, in company with the nitrate and sulphate of potash for a period of 6.364 days (six days, eight hours, forty-four minutes).

The 1 per cent. solution of hydrate of potash gave in eight cells, evaporated two together, 6.93, 6.93, 6.93 and 6.89 grs.; mean 6.92 grs.

The 2 per cent. solution of hydrate of potash gave in three single cells, 6.77, 6.49 and 7.10 grs.; mean 6.79 grs.

The diffusion of nitrate of potash in nine days at the same temperature, as already detailed, was sensibly the same, or 6.83 grs. for both the 1 and 2 per cent. solutions. The times for the two salts were as 1 to 1.4142.

The diffusion of hydrate of potash, at $39^{\circ}7$, may therefore

be stated with reference to that of nitrate of potash, for the selected times, as follows:—

Nitrate of potash, 1 and 2 per cent. solutions	. 100
Hydrate of potash, 1 per cent. solution	. . . 101.3
Hydrate of potash, 2 per cent. solution	. . . 99.4

These experiments at the low temperature concur, therefore, with those made at the higher temperature, in proving that the times of equal diffusion of the two substances have been properly chosen.

[To be continued.]

XXXI. *On Impossible Equations, on Impossible Quantities, and on Tessarines.* By JAMES COCKLE, Esq., M.A., of Trinity College, Cambridge; Barrister-at-Law, of the Middle Temple*.

[Continued from vol. xxxvi. p. 292.]

DEFINITIONS. By an *impossible equation* is meant an equation which has *no root* whatever capable of being expressed in terms of the symbols of the ordinary Double Algebra. By an *impossible quantity* is meant the new species of imaginary by which an impossible equation is supposed to be satisfied.

* Communicated by T. S. Davies, Esq., F.R.S. Lond. and Ed., who adds the following note.

“From my having become accidentally involved in the discussion of ‘congeneric surd equations’ (though merely from having called the attention of Mr. Horner to Garnier’s equation, and not from any contribution of my own towards its elucidation), several of my friends, and some gentlemen who were strangers, have addressed their views on the subject privately to me. Those of Mr. Cockle, from the somewhat close agreement with my own, and from the form suitable for publication in which they were drawn up, I have sent for insertion in the *Philosophical Magazine*. Most others were put in forms that would have required modification for the purpose; and this I did not feel myself at liberty to make, lest I should fail to express in my own language the exact view of the writers. There is one friend, however, a very eminent analyst, who takes a view *directly opposed* to these; and he has given me at different times his own explanation of most of the equations that have been hitherto mooted. When I state that, being opposed to his views (the opposition being founded, as I conceive, on the general principles of analysis), and yet having been uniformly unsuccessful in detecting any specific fallacy in his reasoning, I cannot view the question as being settled. On this account it is that I think the discussion should be kept open; and I trust that the gentleman to whom I refer will afford us the benefit of having his ‘vein of thought’ laid open in his own way. Exceptions and failing cases are always the most instructive subjects of inquiry in every science:—in analysis they always betoken something yet left to be seen or done.

“On one point, however, I wish to be distinctly understood; viz. as not expressing the slightest opinion, *at present*, on the subject of Quaternions, Tessarines, or any of the inquiries into which *i, j, k* enter.”

As by *linear* equations, taken in their utmost generality, we are led to contemplate *negative* quantity; and as by quadratics, cubics, and the higher equations, we are in like manner led to form the notion of *unreal* quantity; so by *surd* or *irrational* equations we may be conducted to the idea of *impossible* quantity.

Let

$$1 + \sqrt{x-4} - \sqrt{x-1} = W,$$

$$1 - \sqrt{x-4} + \sqrt{x-1} = X,$$

$$1 + \sqrt{x-4} + \sqrt{x-1} = Y,$$

$$1 - \sqrt{x-4} - \sqrt{x-1} = Z.$$

Then, by actual multiplication, we obtain

$$6 - 2x + 2\sqrt{x^2 - 5x + 4} = W.X,$$

$$6 - 2x - 2\sqrt{x^2 - 5x + 4} = Y.Z;$$

and hence, on multiplication and reduction,

$$4(5-x) = W.X.Y.Z.$$

Let $WXYZ = V$, then 5 is the only value of x which satisfies $V=0$. Hence, no value of x other than 5 can make either of the factors W , X , Y , or Z equal to zero; for, if so, such other value must make one at least of the remaining factors infinite, and we should have to subject x to incompatible conditions.

Now $W=0$ may, by a transposition, be rendered identical with the equation (1) given by Garnier at p. 335 of the second edition of his *Analyse*, and is satisfied by the value $x=5$. But $X=0$ (which may, by transposition, be rendered identical with the equation numbered (2) by Garnier at the page just cited) is not satisfied by that value; at least, not if we consider the symbol $\sqrt{}$ to be such that the quantities included under it are necessarily affected with $(+1)^2$. This appears to me to be the true meaning of that symbol of radicality, provided that, in the development of algebra from arithmetic, we adhere as closely as we can to the analogies afforded by the latter science, in the most general form of which (universal arithmetic or arithmetical algebra) all the quantities (*i. e.* symbolized *numbers*) employed are, implicitly at least, considered as affected with $(+1)^2$. Accordingly I adhered to this view in forming the equation which led me to my Theory of Tessarines, and by which I sought to connect that theory with ordinary algebra. There is no real loss of generality by thus restricting the symbol $\sqrt{}$. The root corresponding to the

affection $(-1)^2$ may always be obtained by prefixing the sign $-$ to that of radicality.

Waiving this discussion however for the present, let us admit that, in the equation $X=0$, x may have given to it the value $x=5(-1)^2$. Then $X=0$ is obviously satisfied. But the ordinary double algebra is not relieved from its difficulty. For neither $5(+1)^2$ nor $5(-1)^2$ will satisfy either $Y=0$ or $Z=0$. If, in the above instances, the difficulty is to be evaded, it is only by greatly refining our solution, and, as it has occurred to me, by using expressions of the form $m(+1)^2 + n(-1)^2$, and by following certain rules respecting our reductions, and the signs to be affixed to the radicals. To those who would attempt such a complex and artificial system of solution rather than admit the existence of an impossible equation I may hereafter address some observations. They will however probably find, as I have done, that their attempts are unsatisfactory, and the results not philosophically admissible. But I shall here content myself with remarking, that, by any system of rules, however artificial, the difficulty is only thrown further back. Thus, the equation

$$\sqrt{x} + \sqrt{x+1} = 0$$

is utterly intractable.

Should the restricted view which I have taken of the symbol $\sqrt{}$ be deemed ultimately inadmissible, it would not be difficult to frame a new impossible equation, other than that which I have employed in my Theory of Tessarines, and which should give us a new imaginary, determined, like the unreal quantities of double algebra, by means of an *equation*, and so constituting a natural extension of that algebra. In reframing the fundamental equation of the Tessarine Theory, or in adopting one which should give rise to a different theory, geometric interpretation should always be borne in mind, and the uniaxal geometry thence arising would be as *direct* an application of algebra as that which occurs in interpreting and applying the ordinary double algebra.

2 Pump Court, Temple,
August 30, 1850.

XXXII. On the Magnetism of Steam.

By REUBEN PHILLIPS, Esq.

[Continued from vol. xxxvi. p. 511.]

130. **T**HE galvanoscope used in the following experiments is a modification of that formerly described (2.). Instead of two astatic needles, I have employed one sus-

pendent needle, with its directive force partly neutralized by placing near it another fixed magnet. In some experiments this was done by attaching a magnetized needle to a piece of copper wire sliding through a cork stuck on the inside of the beaker; this fixed needle lay horizontally under the suspended needle, and the degree to which it was necessary to balance the earth's magnetism was effected by means of the copper wire. As this adjustment, although in other respects excellent, could not be executed with sufficient rapidity, the needle was subsequently removed from the copper wire, and the swinging needle was partly balanced by a steel magnet placed at a distance. When the fixed magnet was applied, it brought the marked end of the suspended needle to point about 20 or 30 degrees west of the position which it otherwise took up; the needle of the galvanoscope stood rectangularly to the axis of the coils and on the east side. The swinging needle was 1·8 inch long, and its point, which was observed in the microscope as before (3, 4.), pointed to the south. A and C stand for the same sides of the field of view, and the general mode of using the instrument and the zinc screen was as formerly. It may be as well to mention that the microscope inverted. The steam was usually employed at pressures ranging from 15 to 25 lbs. on the inch, the magnetism, so far as my experiments with coils have yet gone, being sensibly the same between these limits. A principal reason for using these lower pressures was, that the temporary joints and pewter coils of the discharging apparatus were much more easily kept in order than at the higher pressures. The stop-cocks, T-pieces, &c., were such as are generally used about oxy-hydrogen blowpipes, and care was taken that they should have sufficient steam-way. The temporary joints were made as follows: having adequately supported and brought together the two ends which it was desired to unite, a strip of *common* sheet caoutchouc about $\frac{1}{2}$ inch wide was wound once round the tubes at the juncture, and then a strip of oiled silk about an inch wide was wound twice round outside the caoutchouc and slightly bound on with fine thread. Three or four rounds of a piece of tinfoil rather wider than the silk were now laid on and carefully rubbed down smooth, and then well bound on with strong thread.

131. A pewter tube 5 feet 7 inches long, having $\frac{3}{20}$ inch for its internal, and $\frac{6}{20}$ inch for its external diameter, was inserted in another pewter tube rather shorter, having an internal diameter about $\frac{7}{20}$ inch and an external diameter of $\frac{9}{20}$ inch; the whole was then made into a dense cylindrical coil, 3·3 inches long, having a cylindrical space 1·2 inch diameter to receive an iron

core. The smaller pewter tube projected from the larger to the extent of about 2 inches at one end and $\frac{1}{3}$ inch at the other; I shall call these the longer and shorter ends of the pipes. A T-piece was now placed on the longer end of the pipes, so that while it could receive the discharge from the larger pipe, the smaller pipe was allowed to stand through the straight way of the T-piece; a stop-cock was placed on the branch of the T-piece, which therefore served to regulate the flow of steam through the outer coil. The end of the smaller pipe after its exit from the T-piece was also furnished with a cock, having a steam-way $\frac{3}{16}$ inch diameter; I call this stop-cock S. The shorter end of the pipes terminated in a short brass connecting piece, the steam-way of which was much larger than the combined steam-ways of the coil, and this connecting piece served to unite the coils with the condenser of the hydro-electric machine. The condenser was interposed between the boiler and the coils during all these experiments, but it was always dry, except where it is said that water was placed in it. The coil having been thus connected with the boiler, one end of a piece of soft iron, 1 inch diameter and 8 inches long, was inserted in the coil; the iron was supported quite independently of the coil, and the axis of the iron and coil of course lay in a horizontal plane. Supposing the back of a watch to have been placed against the east end of the iron core, the steam circulated in a contrary direction to the motion of the hands of the watch.

132. The stop-cock on the branch of the T-piece was closed and S opened, then, the coil being cool, on opening the cock of the boiler, the needle flew across the field of view towards C; with a single puff the motion was perhaps through an angle of 5° .

133. The stop-cock on the branch of the T-piece was opened a little, and the cock of the boiler was opened; an agitation of the needle, or a swing to C, was always observed; as soon as it subsided and steam issued from the branch stop-cock, S was opened, the needle immediately started off to A; the swing, although less than the foregoing, could easily be made by successive puffs quite visible to the unassisted eye.

134. The iron was now removed from the coil, and the last experiment repeated. On opening S the needle went towards A, and by successive puffs it could be vibrated across the field of view and checked by inverse puffs, but the amount of motion was much less than when the iron was in the coil; thus showing that the iron had been magnetized by this apparently new magnetic force. The swing to C was also much reduced (26.). Water was now placed in the condenser, and I think the swing to A became more powerful in consequence.

135. The brass connecting piece which united the short end of the pipes to the condenser was taken away from the coil, and another T-piece was now placed on the other end of S, and the stop-cock removed from the branch of the former T-piece; the branches of these T-pieces stood parallel and were united by a piece of pewter tube bent like the letter U. The last-added T-piece was united to the boiler. By this arrangement it will be seen, that on opening the cock of the boiler the steam could always enter the outer coil, but not the inner coil unless S was open. The iron core was placed in the coil, and the steam circulated in the same direction as before. When the steam was turned on the swing was to A, whether S was open or not, but stronger when S was open. I thought too much steam escaped from between the two pipes at their shorter end, notwithstanding the end of the outer pipe had been considerably contracted; this discharge was sufficiently diminished by twisting a quantity of thread about the place where the larger pipe ended.

136. The coil being cool and S closed, the cock of the boiler was opened; the needle was much agitated, being driven a little in one direction and then in the other very irregularly, but in the main towards A. As soon as the needle became tolerably steady I began to open and shut S at the alternate vibrations of the needle, but could not obtain a certain swing in either direction. The stop-cock S was now closed, and afterwards the cock of the boiler, then S was immediately opened, which sent the needle rather strongly towards C.

137. Another stop-cock, V, was now placed at the shorter end of the pipes, and affixed so as to receive the steam only from the inside coil; the steam could escape from between the two coils as in the last experiment (136.). The cocks S and V were thrown open; there was a strong swing to A as before, when the cock of the boiler was opened. S was now shut, and after sufficient time had elapsed to allow the coil to become well heated, on working S with each alternate vibration, a scarcely perceptible swing to A was produced when S was opened, which swing-producing power generally went off after some puffs. V was now closed and the cock of the boiler and S opened. On opening V the needle started for C. Both the swings to A and C were as strong as before (132, 133.), but regarding the direction of the steam, it is to be observed they were inverted.

138. The stop-cock V was now so united to the shorter end of the pipes, as to receive the discharge from between the coils as well as that of the inside coil. The steam circulated in the same direction as the hands of the watch, and the iron core was still in the coil. When the cock of the boiler was

opened, S and V being previously opened, the needle swung to C; and to A when the cock of the boiler and S were opened, and V then opened. When the iron was removed, the swings both to C and A were much diminished.

139. The coil was removed from the condenser and turned about in a direction parallel to the needle, so that the steam could enter through V and escape through the combination of T-pieces at the other end of the pipes; the steam being, in fact, blown through the coils in a direction opposite to that of the last experiment. Opening V and S, and then opening the cock of the boiler, gave a swing to C. Opening V and the cock of the boiler, and then letting out the steam by opening S, gave the swing to A. The iron core was not used with this arrangement.

140. The coil was moved through an angle of 180° as before (19.). Both swings were now reversed, as compared with the former experiment; that is, using the steam as before, the swing to C became a swing to A, and the swing to A became a swing to C.

141. The pewter coil (29.) was united to the condenser by a piece of brass, the steam-way of which was of larger diameter than the bore of the coil. The steam circulated in a contrary direction to the hands of the watch. When the cock of the boiler was opened, the swing was to A and rather strong.

142. The other end of the pipe of the coil was united by the same piece of brass to the condenser, and the steam circulated in the same direction as the hands of the watch; the swing was still to A.

143. A stop-cock was now placed on one end of the tube of this pewter coil, and the other end was affixed to the condenser, as in the two preceding experiments; the steam circulated in a direction contrary to the hands of the watch; also the iron core was placed in the coil. The stop-cock being open and the coil cool, turning on the steam produced a strong swing to C. The stop-cock was now closed with the cock of the boiler open; on opening the stop-cock at the proper positions of the needle, a swing to A was obtained on opening the stop-cock, but it was not nearly so strong as the corresponding swing with the double coil. The diameter of the steam-way of the stop-cock was $\frac{7}{10}$ inch.

144. The iron was now removed. The swing to C when the coil was cool, although less, was still very good, as one puff sent the edge of the needle rather more than the whole length of the micrometrical scale. The swing to A was, however, very feeble; and it required about ten puffs to produce a swing one-third the length of the scale.

145. A glass tube 28 inches long was formed into a coil of five convolutions. This glass tube happened to be rather conical; its internal diameter at one end being $\frac{7}{40}$ inch, and at the other end about $\frac{6}{40}$ inch, and the tube was $\frac{9}{40}$ inch external diameter. The end of the tube having the larger diameter was united to the condenser, and the iron was placed in the coil; the steam circulated in a direction contrary to that of the hands of the watch. In these experiments, 10 lbs. on the inch was found to be an advantageous pressure for the steam. On opening the cock of the boiler the needle moved towards C; the steam being then shut off, the needle made a sudden start towards A; and I think the force which sent the needle towards A was greater than that which moved it to C. Three puffs of steam sent the needle nearly across the field of view.

146. The iron was now removed, the swing remained much as before; if there was any difference, I think it was rather greater without the iron. After many experiments I thought I could perceive that the needle, when the iron was in, did not so promptly obey the steam as when the iron was out, but seemed to move rather sluggishly, as if the iron resisted the production of the magnetic force of the coil.

147. The end of the tube having the smaller diameter was now united to the condenser by the same piece of brass which was used in the preceding experiment, and the steam circulated in the same direction. The swing, when the steam was turned on, was to C, but much more feeble than before, and the coil worked better at a higher pressure. I could not perceive that the iron varied the magnetic energy of the coil.

148. It immediately follows from the foregoing experiments (132, 135, &c.), that the direction of the magnetism imparted to a coil is not directly, if indeed it is at all, connected with the direction of the motion of the steam. In these experiments two magnetic forces are observable, always opposite in direction and about equal in power: one of these I have before shown to be intimately connected with condensation, the other is produced under circumstances where condensation cannot take place, but where evaporation does. The experiments (136, 137.) show very conclusively that the friction of steam along a pipe, apart from condensation or evaporation, produces no magnetic effect. A more complete consideration of all these experiments must be deferred until I have finished a further set.

XXXIII. *On a Geometrical Theorem.*

By WILLIAM SPOTTISWOODE, *M.A., of Balliol College, Oxford**.

IF three cones of the second order, having a common vertex, cut one another two and two in at least three straight lines, and if in each cone there be inscribed a hexahedral angle, such that each of the nine straight lines of intersection shall be a common edge of two hexahedral angles; then, adopting a former notation, the equations to the three cones may be written thus:—

$$\left. \begin{aligned} S(V.V\lambda_1\lambda_2.V\mu_2\nu_1.V.V\mu_1\mu_2.V\nu_2\lambda_1.V.V\nu_1\nu_2.V\lambda_2\mu_1) &= 0 \\ S(V.V\lambda_2\lambda.V\mu\nu_2.V.V\mu_2\mu.V\nu\lambda_2.V.V\nu_2\nu.V\lambda\mu_2) &= 0 \\ S(V.V\lambda\lambda_1.V\mu_1\nu.V.V\mu\mu_1.V\nu_1\lambda.V.V\nu\nu_1.V.\lambda_1\mu) &= 0 \end{aligned} \right\}, \quad (1.)$$

or

$$\left. \begin{aligned} S\lambda_1\lambda_2\mu_2.S\mu_1\mu_2\nu_1.S\nu_1\nu_2\lambda_2.S\lambda_1\mu_1\nu_2 &= S\lambda_1\lambda_2\nu_2.S\mu_1\mu_2\lambda_1.S\nu_1\nu_2\mu_1.S\lambda_2\mu_2\nu_1 \\ S\lambda_2\lambda\mu.S\mu_2\mu\nu_2.S\nu_2\nu\lambda.S\lambda_2\mu_2\nu &= S\lambda_2\lambda\nu.S\mu_2\mu\lambda_2.S\nu_2\nu\mu_2.S\lambda\mu\nu_2 \\ S\lambda\lambda_1\mu_1.S\mu\mu_1\nu.S\nu\nu_1\lambda_1.S\lambda\mu\nu_1 &= S\lambda\lambda_1\nu_1.S\mu\mu_1\lambda.S\nu\nu_1\mu.S\lambda_1\mu_1\nu \end{aligned} \right\}; \quad (2.)$$

and if these be written thus,

$$U = W, \quad U_1 = W_1, \quad U_2 = W_2, \quad . \quad . \quad . \quad (3.)$$

the equation

$$UU_1U_2 = WW_1W_2 \quad . \quad . \quad . \quad . \quad (4.)$$

will be of the fourth order in each of the vectors $\lambda, \lambda_1, \lambda_2, \dots$, and will be satisfied when any of them (*e. g.* ν) is made to coincide successively with each of the others; (4.) is consequently the equation of a cone of the fourth order, on which all the nine edges lie. Hence the following

THEOREM. *If three cones of the second order, having a common vertex, intersect one another two and two, the nine lines of intersection (three being selected from each pair of cones) will lie on a cone of the fourth order.*

Raigmore, Aug. 27, 1850.

XXXIV. *On the Chemical Formula of the Nitroprussides.*

By JOHN KYD†.

AT the suggestion of Professor Will, I have made some experiments upon the nitroprussides recently described by Dr. Playfair, mainly with the view of testing the simple and elegant formula which Dr. Playfair considers as the probable one, although his experiments do not permit him to

* Communicated by the Author.

† From Liebig's *Annalen*, vol. lxxiv. p. 340.

regard it as the expression of the analytical results. I selected for this purpose the soda salt, which is the most easily crystallized. The impure nitroprusside of sodium, which was obtained in solution by the saturation of the acid with carbonate of soda, was precipitated by the addition of sulphate of copper in the form of nitroprusside of copper; this was thoroughly washed with water, and lastly carefully decomposed by caustic soda. The solution of nitroprusside of sodium thus obtained was separated by filtration from the oxide of copper liberated, and evaporated at a gentle heat; large crystals were then obtained, which I submitted to analysis.

The iron and soda were estimated in the form of peroxide of iron and sulphate of soda, after the decomposition of the salt with concentrated sulphuric acid.

I. 0.531 grm. gave 0.1490 grm. of peroxide of iron and 0.2505 grm. of sulphate of soda.

II. 0.471 grm. gave 0.1330 grm. of peroxide of iron and 0.2223 grm. of sulphate of soda.

Regarding the combustions, the first was made with chromate of lead, the second with peroxide of copper and oxygen gas.

I. 0.5157 grm. gave 0.3937 grm. of carbonic acid and 0.0610 grm. of water.

II. 0.4394 grm. gave 0.3265 grm. of carbonic acid and 0.0526 grm. of water.

The determination of the nitrogen was made according to the method of Dumas: 0.298 grm. gave 74 cub. cent. of moist nitrogen at 57° F., and with the barometer at 28".

I shall now compare these results with those of the composition calculated according to the simplest formula given by Playfair, and the mean of the same chemist's analyses of the soda salt obtained by the same method.

	Eq.	Calc.	Found.			Mean of Playfair's analyses.
			I.	II.	Mean.	
Iron . . .	2	19.48	19.64	19.77	19.70	19.33
Sodium . . .	2	15.98	15.41	15.42	15.42	15.48
Carbon . . .	10	20.66	20.82	20.26	20.54	20.17
Hydrogen . . .	4	1.38	1.31	1.33	1.32	1.53
Nitrogen . . .	6	28.93	29.35	...	29.35	27.78
Oxygen . . .	5	13.77	...	...	13.66	
		100.00			100.00	

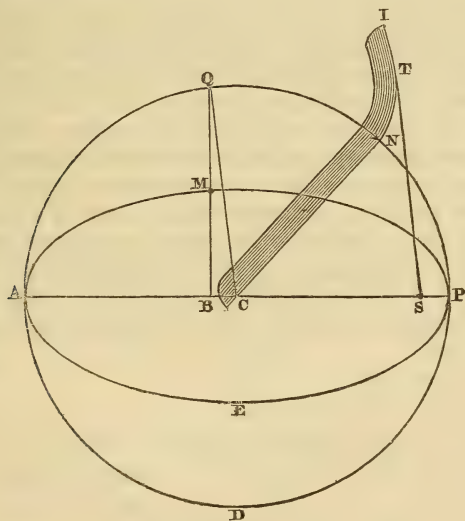
Playfair's more complicated formula, $\text{Fe}^5 \text{Cy}^{12}, 3\text{NO}, 5\text{Na} + 10\text{HO}$, requires in 100 parts, 19.33 Fe, 16.02 Na, 19.89 C, 1.38 H, and 29.00 N. It is evident, therefore, that in regard to the amount of soda and carbon, the analysis corresponds

best with the more simple formula $\text{Fe}^2 \text{Cy}^5 \text{NO}$, $2\text{Na} + 4\text{HO}$. The other elements, viz. the iron, the hydrogen and the nitrogen, when calculated according to both formulæ, so nearly coincide, that the difference cannot be ascertained with certainty by analysis. The amount of carbon appears to me decisive; because, as calculated according to both formulæ, it exhibits the greatest difference, and the quantity of carbon found is decidedly in favour of the more simple formula.

XXXV. *On the Anomaly-Ruler ; an Instrument to assist in the graphic representation of the place of a Gravitating Projectile in an Elliptic Orbit.* By WILLIAM JOHN MACQUORN RANKINE, C.E., F.R.S.E., F.R.S.S.A. &c.*

IT may sometimes be desirable, in lecturing on or in discussing the motions of comets, to possess the means of easily and rapidly laying down on a diagram, with as much accuracy as the scale of the drawing will permit, the places of such bodies in their elliptic orbits at given instants of time. The instrument which I shall now describe is intended to facilitate this operation.

It is simply a thin flat ruler, the form of which is represented by the shaded figure in the drawing, and which may be made of any convenient size.



The fiducial edge, from C to N, is straight. Those two points ought to be marked by fine transverse lines on both faces of the ruler. From N to I the fiducial edge has the

* Communicated by the Author.

form of the involute of the circle whose radius is CN, and centre C. The length of the curved portion of the ruler should be somewhat more than one-half of that of the straight part.

The use of this instrument is as follows:—

Problem.—The elliptic orbit of a body, gravitating towards a given focus, being given, to represent on a diagram the place of the body corresponding to a given mean anomaly.

Solution.—About a centre C with the radius CN describe the circle PNQADP, and let its diameter AP be taken to represent the major axis of the given elliptic orbit. Let S represent the focus of attraction, P the perihelion, and the angle PCN the given mean anomaly. Apply the anomaly-ruler to the diagram as shown in the drawing, so that the mark C shall be over the centre of the circle, the mark N at the end of the arc of mean anomaly, and the convexity of the involute NI towards the perihelion. From the focus S draw a straight line ST touching the involute NI. Through the centre C draw a straight line CQ parallel to ST and on the same side of AP. From the point Q, where CQ cuts the circle, draw a straight line QB perpendicular to AP; divide this line by the point M in such a proportion that $BQ : BM ::$ major axis : minor axis of the elliptic orbit. Then the point M represents the place of the body corresponding to the mean anomaly PCN. Q. E. I.

The proof of this solution follows immediately from the 31st proposition of the first book of Newton's Principia.

If the entire ellipse PMAEP, similar to the given orbit, can be described with sufficient accuracy, the point M may be determined by the intersection of the ordinate BQ with the ellipse, without performing the proportional division of that line.

It has been suggested by the Astronomer Royal for Scotland, that by means of a turning-lathe a series of ellipses might be engraved on a plate with the same major axis, varying in excentricity between a circle and a straight line; and that impressions of this plate might be used in the graphic representation of cometic motions, with the aid of an anomaly-ruler of suitable size.

Glasgow, August 1850.

XXXVI. On *Bisignal Univalent Imaginaries*.

By the Rev. THOMAS P. KIRKMAN, M.A.*

IF *abcdefghijklmnop* be fifteen imaginaries having no linear relation to each other, and such that

$$a^2 = b^2 = \dots = p^2 = -1,$$

and also that by definition every pair of them, as *m* and *n*,

* Communicated by the Author.

possesses the property expressed by the equation $mn + nm = 0$, we can form thirty-five triplets thus, so as to employ once every duad that can be made with the fifteen symbols:

$$\begin{array}{l}
 \text{A} \left\{ \begin{array}{lll} abc \\ ade & -bdf & cdg \\ afg & beg & cef \end{array} \right. \\
 \\
 \text{B} \left\{ \begin{array}{llllll} ahi & bhk & -chl & dhm & ehn & -fho & ghp \\ akl & -bil & -cik & -din & eim & fip & gio \\ amn & bmo & cmp & -dko & ekp & -fkm & -gkn \\ aop & -bnp & cno & dlp & elo & fln & -glm. \end{array} \right.
 \end{array}$$

If we consider every triplet taken with its sign to be equal to negative unity, as

$$abc = -bdf = -1,$$

and to imply three equations, such as

$$a = bc, \quad b = ca, \quad c = ab; \quad -b = df, \quad -d = fb, \quad -f = bd;$$

equivalent to the equations

$$a = -cb, \quad b = -ac, \quad c = -ba; \quad b = fd, \quad d = bf, \quad f = db;$$

we shall find that the three equal values of each of the seven imaginaries, $abcdefgh$, deduced from the first seven triplets A, are all congruous with all; and that the seven equal values of each of the eight imaginaries, $hiklmnop$, deduced from the twenty-eight remaining triplets B, are all congruous with all. Thus the two values of h , given by the triplets ahi and $-chl$,

$$h = ia = cl,$$

are congruous with the values of k , given by the triplets $-cik$ and akl ,

$$k = ic = la;$$

for $ic = la$ follows from $ia = cl$.

But if we compare the values of the first seven imaginaries, as given by the first seven triplets, with those deduced from the other twenty-eight, we find them contradictory. From the triplets dhm and eim , we have

$$m = dh = ei,$$

giving

$$de = ih = -hi,$$

or, comparing ade ,

$$a = -hi;$$

from bhk and $-cik$, we get

$$k = bh = ic,$$

giving

$$bc = hi,$$

or, comparing abc ,

$$a = hi;$$

a pair of results that can be reconciled on no less violent supposition, than that the duad imaginary hi retains its value when it changes its sign: and I have shown, at page 450 of the thirty-third volume of this Magazine, that no one of the twenty-seven triplets following ahi can be admitted along with it, if we interpret each to express three such conditions as $a = hi$, $h = ia$, $i = ah$. It is there proved impossible to make consistently the 15×7 suppositions, such as $ab = c$, $ac = -b$, $ad = e$, &c., which are necessary to enable us to eliminate the duads ab , ac , ad , &c. from the product of two pluquaternions of fifteen imaginaries,

$$Q_{15} = w + aa + bb + cc + dd + ee + ff + gg + hh + ii + kk + ll \\ + mm + nn + oo + pp,$$

and

$$Q'_{15} = w_i + aa_i + bb_i + \dots + pp_i,$$

so as to reduce the product to the form

$$Q''_{15} = w_{ii} + aa_{ii} + bb_{ii} + \dots + pp_{ii}.$$

From this consideration may be framed a proof of a celebrated negative, which has not yet lost its interest, although the question has been some time ago set at rest by a master of analysis, who has achieved the laborious task of establishing the negative by ordinary algebra. See a memoir in the Transactions of the Royal Irish Academy, vol. xxi., "On an extension of a Theorem of Euler, &c., by J. R. Young, Professor of Mathematics in Belfast College," a gentleman in whose recent and most cruel wrongs every cultivator of science in this empire has been bitterly insulted. The proposition meant is, that "Two sums each of sixteen arbitrary algebraic squares cannot have for their product a sum of sixteen algebraic squares."

Let it be supposed, for a moment, that they can, or that

$$(w^2 + a^2 + b^2 + \dots + p^2)(w_i^2 + a_i^2 + b_i^2 + \dots + p_i^2) \\ = (w_{ii}^2 + a_{ii}^2 + b_{ii}^2 + \dots + p_{ii}^2),$$

the number of squares in each factor being sixteen. The function

$$w^2 + a^2 + b^2 + \dots + p^2$$

is the product of two pluquaternions,

$$Q_{15} = w + aa + bb + \dots + pp,$$

and

$$Q_{-15} = w - aa - bb - \dots - pp,$$

Q_{-15} denoting a function differing from Q_{15} only in the signs of the fifteen imaginaries $abc \dots p$, which have the properties above defined. We have then, as an equivalent form of the above equation,

$$Q_{15} Q_{-15} Q'_{15} Q'_{-15} = Q''_{15} Q''_{-15};$$

or, since $Q'_{15} Q'_{-15}$ is real,

$$Q_{15} Q'_{15} Q'_{-15} Q_{-15} = Q''_{15} Q''_{-15}.$$

The left member of our supposed equation is thus divided into a pair of factors, $Q_{15} Q'_{15}$ and $Q_{-15} Q'_{-15}$, which differ only in the signs of the fifteen imaginaries; and since no such pair of factors can be found to produce the right member, different in form as to the imaginaries from Q''_{15} and Q''_{-15} , we must have

$$Q_{15} Q'_{15} = Q''_{15},$$

$$Q_{-15} Q'_{-15} = Q''_{-15},$$

congruous equations, considered as functions of our imaginaries: but both these are proved above to be contradictory. The negative proposition seems thus established: but all this involves the petition of this other negative, that our factors Q cannot be formed with less than fifteen monad imaginaries. Why not with nine monads and six duads, as at page 453 of vol. xxxiii.?

Let us return to our refractory triplets, and try to induce the imaginaries to submit to the violence above hinted at. If in any argument it suits our convenience, for the sake of clearness or symmetry, to retain a term AZ , in which A is not infinite, and Z is zero, we may exhibit it at different points of the process with different signs; for AZ is a quantity which changes not its value when we reverse its sign. Suppose that A is not a symbol of real quantity, but given in value by the equation $A=B$, when B is an imaginary having one value only: if there is no term in our reasoning that is affected by our definition of A , except AZ , we may, on reviewing our process, in which AZ appears with contrary signs at different points, conceive that the property, which the term has, of retaining its value when its sign is changed, is conferred on it by A , considered to be, whether affected by a positive or negative sign, constantly equivalent to $+B$: and evidently, if it facilitates the establishment of relations between A and other like quantities, considered apart from real numbers, we may endow A by definition with the property, that its value is inde-

pendent of its sign, provided that we take care in our result, to reduce to zero every term which is affected by the value or sign of A.

Let me beg the reader's forbearance for a little while, and his permission to lay down, for brevity's sake, the definition following:—

A *bisignal univalent*, or more briefly, a *bisignal*, is a quantity whose value is independent of its sign; and such that it may be employed in the same argument with contrary signs, and yet remain always equivalent to one defined value.

In real arithmetic there can be no bisignals, except zero and its reciprocal; in imaginary arithmetic we may establish any number, each having a value neither null nor infinite, if they are not suffered to affect any finite terms of our results.

We shall then venture to define, that all the twenty-eight duads, *hi, hk, hl, &c.*, made with the imaginaries *hiklmnop* are bisignal imaginaries, whose values are given by the twenty-eight lower triplets: and we have now the power of asserting, at one point of our argument, that $a=hi$ and not $=ih$; and again, at another point of the argument, that $a=ih$ and not $=hi$. Through all this we must retain the definition that $hi+ih=0$, which is the common property of all our imaginaries, by an extension of Sir W. R. Hamilton's definition of the three imaginaries of his quaternion theory. I am convinced that it is in vain to attempt to *prove* this property, at least in the case of those imaginaries which are not combined into a congruous system of seven. We are to conceive, then, that whenever we change for our convenience the sign of *hi*, we change by the same supposition the sign of *ih*; and that these obliging duads are always of contrary signs.

By this definition of the twenty-eight duads, *hi, &c.*, we remove all contradiction among our triplets, *if only* the reader will grant that the definition itself is not a flat contradiction. There is no need that we should swallow the absurdity that *hi* is of two opposite signs at once, *i. e.* in the same circumstances: when we are about to compare, as in a preceding paragraph, the four triplets *dhm, eim, ade* and *ahi*, we establish the proposition $a=ih=-hi$; and the result of our comparison agrees with this: at another moment, in other circumstances, when we compare together the four triplets *bhk, -cik, abc* and *ahi*, we first lay down that $a=hi=-ih$, and our way is clear again. We are in a condition to bid defiance to every difficulty about the comparison of the first seven triplets with the lower group of twenty-eight; and we proceed without fear to the formation of our sixteen functions, $w_{\mu}a_{\mu}b_{\mu}$ &c., which are to appear on the right side of the equation

$$\begin{aligned}
 & (w + aa + bb + cc + \dots + pp)(w_i + aa_i + bb_i + cc_i + \dots + pp_i) \\
 & = w_{ii} + aa_{ii} + bb_{ii} + cc_{ii} + dd_{ii} + ee_{ii} + ff_{ii} + gg_{ii} + hh_{ii} + ii_{ii} \\
 & + kk_{ii} + ll_{ii} + mm_{ii} + nn_{ii} + oo_{ii} + pp_{ii};
 \end{aligned}$$

from the congruity of which must follow at once

$$\begin{aligned}
 & (w^2 + a^2 + b^2 + c^2 + \dots + p^2) \cdot (w_i^2 + a_i^2 + b_i^2 + c_i^2 + \dots + p_i^2) \\
 & = w_{ii}^2 + a_{ii}^2 + b_{ii}^2 + \dots + p_{ii}^2.
 \end{aligned}$$

The values of a and k , for example, given by the triplets, are

$$a = bc = de = fg = (hi) = (kl) = (mn) = (op),$$

$$k = la = bh = ic = do = pe = fm = gn;$$

where the bisignals are bracketed for distinction, as are also the terms introduced by them in the sixteen functions following:

$$\begin{aligned}
 w_{ii} = & ww_i - aa_i - bb_i - cc_i - dd_i - ee_i - ff_i - gg_i - hh_i - ii_i - kk_i \\
 & - ll_i - mm_i - nn_i - oo_i - pp_i,
 \end{aligned}$$

$$\begin{aligned}
 a_{ii} = & aw_i + wa_i + bc_i - cb_i + de_i - ed_i + fg_i - gf_i + (hi_i - ih_i) \\
 & + (kl_i - lk_i) + (mn_i - nm_i) + (op_i - po_i),
 \end{aligned}$$

$$\begin{aligned}
 b_{ii} = & bw_i + wb_i + ca_i - ac_i + fd_i - df_i + eg_i - ge_i + (hk_i - kh_i) \\
 & + (li_i - il_i) + (mo_i - om_i) + (pn_i - np_i),
 \end{aligned}$$

$$\begin{aligned}
 c_{ii} = & cw_i + wc_i + ab_i - ba_i + dg_i - gd_i + ef_i - fe_i - (hl_i - lh_i) \\
 & - (ik_i - ki_i) + (mp_i - pm_i) + (no_i - on_i),
 \end{aligned}$$

$$\begin{aligned}
 d_{ii} = & dw_i + wd_i + ea_i - ae_i + bf_i - fb_i + gc_i - cg_i + (hm_i - mh_i) \\
 & - (in_i - ni_i) - (ko_i - ok_i) + (lp_i - pl_i),
 \end{aligned}$$

$$\begin{aligned}
 e_{ii} = & ew_i + we_i + ad_i - da_i + gb_i - bg_i + fc_i - cf_i + (hn_i - nh_i) \\
 & + (im_i - mi_i) + (kp_i - pk_i) + (lo_i - ol_i),
 \end{aligned}$$

$$\begin{aligned}
 f_{ii} = & fw_i + wf_i + ga_i - ag_i - bd_i + db_i + ce_i - ec_i - (ho_i - oh_i) \\
 & + (ip_i - pi_i) + (ln_i - nl_i) - (km_i - mk_i),
 \end{aligned}$$

$$\begin{aligned}
 g_{ii} = & gw_i + wg_i + af_i - fa_i + be_i - eb_i + cd_i - dc_i + (hp_i - ph_i) \\
 & + (io_i - oi_i) - (kn_i - nk_i) - (lm_i - ml_i),
 \end{aligned}$$

$$\begin{aligned}
 h_{ii} = & hw_i + wh_i + ia_i - ai_i + kb_i - bk_i + cl_i - lc_i + md_i - dm_i \\
 & + ne_i - en_i + fo_i - of_i + pg_i - gp_i,
 \end{aligned}$$

$$\begin{aligned}
 i_{ii} = & iw_i + wi_i + ah_i - ha_i + bl_i - lb_i + ck_i - kc_i + dn_i - nd_i \\
 & + me_i - em_i + pf_i - fp_i + og_i - go_i,
 \end{aligned}$$

$$\begin{aligned}
 k_{ii} = & kw_i + wk_i + la_i - al_i + bh_i - hb_i + ic_i - ci_i + do_i - od_i \\
 & + pe_i - ep_i + fm_i - mf_i + gn_i - ng_i,
 \end{aligned}$$

$$\begin{aligned}
 l_{ii} = & lw_i + wl_i + ak_i - ka_i + ib_i - bi_i + hc_i - ch_i + pd_i - dp_i \\
 & + oe_i - eo_i + nf_i - fn_i + gm_i - mg_i,
 \end{aligned}$$

$$m_{||} = mw_i + wm_i + na_i - an_i + ob_i - bo_i + pc_i - cp_i + dh_i - hd_i \\ + ei_i - ie_i + kf_i - fk_i + lg_i - gl_i,$$

$$n_{||} = nw_i + wn_i + am_i - ma_i + bp_i - pb_i + oc_i - co_i + id_i - di_i \\ + eh_i - he_i + fl_i - lf_i + kg_i - gk_i,$$

$$o_{||} = ow_i + wo_i + pa_i - ap_i + bm_i - mb_i + cn_i - nc_i + kd_i - dk_i \\ + el_i - le_i + hf_i - fh_i + gi_i - ig_i,$$

$$p_{||} = pw_i + wp_i + ao_i - oa_i + nb_i - bn_i + cm_i - mc_i + dl_i - ld_i \\ + ek_i - ke_i + fi_i - if_i + gh_i - hg_i.$$

The bracketed terms, of which there is one, and one only, introduced by each bisignal, must severally vanish, giving the seven conditions,

$$h : h_i = i : i_i = k : k_i = l : l_i = m : m_i = n : n_i = o : o_i = p : p_i,$$

as those which must be fulfilled in order that our results may be consistent with our definitions; and these seven conditions ought to be sufficient, if our reasoning is valid, in order that

$$(w^2 + a^2 + b^2 + c^2 + d^2 + e^2 + f^2 + g^2 + h^2 + i^2 + k^2 + l^2 + m^2 + n^2 \\ + o^2 + p^2)$$

$$\times (w_i^2 + a_i^2 + b_i^2 + c_i^2 + d_i^2 + e_i^2 + f_i^2 + g_i^2 + h_i^2 + i_i^2 + k_i^2 + l_i^2 \\ + m_i^2 + n_i^2 + o_i^2 + p_i^2)$$

should be identical with

$$w_{||}^2 + a_{||}^2 + b_{||}^2 + c_{||}^2 + d_{||}^2 + e_{||}^2 + f_{||}^2 + g_{||}^2 + h_{||}^2 + i_{||}^2 + k_{||}^2 + l_{||}^2 \\ + m_{||}^2 + n_{||}^2 + o_{||}^2 + p_{||}^2;$$

or that the product of two sums, each of sixteen algebraic squares, should be a sum of sixteen algebraic squares.

That they are sufficient, is evident from inspection, and is in fact a known truth, discovered by Prof. J. R. Young, and established by ocular demonstration in the memoir already referred to; although the author gives no account of the process by which he arrived at the result. Should the reader hesitate to accept this conclusion on the strength of my arguments, from a very natural suspicion that the latter are more to be admired for their luck than their learning, I trust that he will allow the result to be an apology for the reasoning, and be lenient to the logic for the conclusion's sake. Is there not room to indulge the hope, that when the theory of these imaginaries is perfectly understood, as instruments of the *algebra of time and order*, what is here offered on *bisignal univalents* may amount to something better than fortunate nonsense and two useless words?

Let us now consider two pluquaternions, each of twenty-three imaginaries, viz. the fifteen before us and the eight

$h'i'k'l'm'n'o'p'$, all having the property $h'^2=i'^2=\dots=p'^2=-1$, and such that $m'n'+n'm'=0$ for every pair; and ask how many conditions are to be added to the seven already found in order that $Q_{23}.Q'_{23}=Q''_{23}$. If we add to the groups of triplets A and B the following group,

$$B' \begin{cases} ah'i' & bh'k' & -ch'l' & dh'm' & eh'n' & -fh'o' & gh'p' \\ ak'l' & -bi'l' & -ci'k' & -di'n' & ei'm' & fi'p' & gi'o' \\ am'n' & +bm'o' & cm'p' & -dk'o' & ek'p' & -fk'm' & -gk'n' \\ ad'p' & -bn'p' & cn'o' & dl'p' & el'o' & fl'n' & -gl'm', \end{cases}$$

we shall obtain, as before, seven congruous values of each of the eight $h'i'k'l'm'n'o'p'$; and by defining every duad imaginary made with these as bisignal, we shall remove all contradiction from the values of $abcdefg$ deduced from the groups A and B'. The equation to be satisfied, $Q_{23}.Q'_{23}=Q''_{23}$, or

$$\begin{aligned} & (w+aa+bb+\dots+ff+gg+hh+h'h'+\ddot{u}+i'i'+kk \\ & \quad +k'k'+\dots+pp+p'p') \\ & \times (w_1+aa_1+bb_1+\dots+ff_1+gg_1+hh_1+h'h'_1+\ddot{u}_1+i'i'_1+kk_1 \\ & \quad +k'k'_1+\dots+pp_1+p'p'_1) \\ & = w_{11}+aa_{11}+bb_{11}+\dots+ff_{11}+gg_{11}+hh_{11}+h'h'_{11}+\ddot{u}_{11}+i'i'_{11}+kk_{11} \\ & \quad +k'k'_{11}+\dots+pp_{11}+p'p'_{11}, \end{aligned}$$

requires that the sixty-four duads, $hh'hi'hk'$ &c., made each from both groups $hi\dots p$ and $h'i'\dots p'$, should be eliminated or otherwise disposed of. Elimination by substitution of a monad imaginary is impracticable; for hk' e. g. cannot be equal to any of the seven $abcdefg$, nor to any of the eight $hik\dots p$, nor to any of $h'i'k\dots p'$, without introducing a linear relation between two of our twenty-three imaginaries, contrary to hypothesis. Thus the supposition $hk'=f$, combined with either of the existing conditions $-ho=f$, $m'k'=f$, would give a linear relation between two monads. These sixty-four duads must then perforce appear in the product $Q_{23}Q'_{23}$, unless they are made to vanish by null coefficients. From our definition of $h'i'$, $h'k'$, &c. as bisignals, it will follow, as before in the case of hi , hk &c. and their coefficients, that the seven equations

$$h':h'_1=i':i'_1=k':k'_1=\dots=p':p'_1$$

must be satisfied; but there is no necessity, from these conditions, that the ratios $h:h_1$ and $h':h'_1$ should be equal. Now the duad hk' will introduce into the product of Q_{23} and Q'_{23} , with one sign or other, the term $hk'_1-k'h_1$, and no more. This will not vanish, unless $h:h_1=k':k'_1=h':h'_1$; but does vanish, if the ratios $h:h_1$ and $h':h'_1$ are equal. As this equality of ratios, being admitted, will cause all the sixty-four duads hh' , hi' , hk' , &c. to disappear, we have our product $Q_{23}.Q'_{23}$

of the required form Q''_{23} ; from which it follows, that the product of two sums each of twenty-four squares can be reduced algebraically to a sum of twenty-four squares, if fifteen conditions are satisfied; or that

$$(w^2 + a^2 + b^2 + \dots + g^2 + h^2 + h'^2 + i^2 + i'^2 + \dots + p^2 + p'^2)(w_i'^2 + a_i'^2 + b_i'^2 + \dots + g_i'^2 + h_i'^2 + h_i'^2 + i_i'^2 + i_i'^2 + \dots + p_i'^2 + p_i'^2)$$

is a sum of twenty-four squares, if

$$h : h_i = h' : h'_i = i : i_i = i' : i'_i = \dots = p : p_i = p' : p'_i.$$

We can readily prove, by adding continually a new group of twenty-eight triplets made with eight new imaginaries, giving rise to twenty-eight new bisignal duads, that the product of two sums each of $8(n+1)$ ($n > 0$) algebraic squares can be reduced to a sum of $8(n+1)$ algebraic squares, if the given squares satisfy $8n-1$ conditions. Hence we have the following theorem :

The product of $(r+1)$ sums, each of $8(n+1)$ algebraic squares ($n > 0$), can be reduced to a sum of $8(n+1)$ algebraic squares if the given roots satisfy $8nr-r$ assignable simple equations.

In the case of 2^n-1 imaginaries, triplets can be formed by which every duad may have an equivalent monad, and this consistently with a law that must pervade every congruous system of such triplets: namely, this law, that if abc and ahi be two triplets, giving values $a=bc=hi$, both $bi=ch$ and $bh=ic$ must hold good; that is, bi and ch must be completed into triplets by the same imaginary m , and bh and ic must be combined with the same monad n .

Such a system is completed for thirty-one imaginaries by adding to the groups A B B' the following, the signs being here of no importance.

$$\begin{array}{l} B'' \left\{ \begin{array}{lllllll} agr & bqs & cqt & dqu & eqv & fqw & gqx \\ ast & brt & crs & drv & eru & frx & grw \\ auv & buw & cux & dsw & esx & fsu & gsv \\ arwx & bvx & crw & dtx & etw & ftv & gtu \end{array} \right. \\ \\ C \left\{ \begin{array}{llllllll} hh'q & ih'r & kh's & lh't & mh'u & nh'v & oh'w & ph'x \\ hi'r & ii'q & ki't & li's & mi'v & ni'u & oi'x & pi'w \\ hk's & ik't & kk'q & lk'r & mk'w & nk'x & ok'u & pk'v \\ hl't & il's & kl'r & ll'q & ml'x & nl'w & ol'v & pl'u \\ hm'u & im'v & km'w & lm'x & mm'q & nm'r & om'r & pm't \\ hn'v & in'u & kn'x & ln'w & mn'r & nn'q & on'q & pn's \\ ho'w & io'x & ko'u & lo'v & mo's & no't & od't & po'r \\ hp'x & ip'w & hp'v & lp'u & mp't & np's & op's & pp'q. \end{array} \right. \end{array}$$

The third vertical row in each of the columns in the group C is formed by writing under its first letter, in order, the multipliers of that letter in the group B". The triplets, *e. g. hm'u, hlc; p'm'c, p'ul; xm'l, xcu*; fulfill the law spoken of. If now under the group of triplets (B'B"C) we write two such groups having the same initial letters, a final group C' can be added in the manner of C, completing the system of triplets made with sixty-three symbols, by all which the law in question will be satisfied.

Croft Rectory, near Warrington,
September 3, 1850.

XXXVII. *Notices respecting New Books.*

Essay on the Theory of Attraction. By JOHN KINNERSLEY SMYTHIES, Barrister-at-Law of the Middle Temple.

THIS paper consists of two distinct parts, geometrical and mechanical: in the former, the author deserves high praise for his accuracy and ingenuity; in the latter, he must bear to be told that he is ingenious, but not accurate.

The geometrical part of the paper consists in a determination of the relation existing among the ten distances of five points in space. This problem was first solved by Carnot, who published a tract on the subject. Mr. Smythies appears not to be aware of this, by his not alluding to Carnot.

The mechanical part is the asserted deduction of a principle which will startle every reader who is competent to be startled: it is, that any five material* particles have a *necessary mathematical relation existing between their distances and central forces independently of their velocities*. The reasoning is of the following kind. The distances between the five points being *a, b, &c.*, the relation existing between them leads to a relation of the form

$$f(a, da, d^2a, b, db, d^2b, \dots) = 0.$$

Where the particles are at rest, say that this becomes

$$\psi(a, d^{1/2}a, b, d^{1/2}b, \dots) = 0.$$

We now quote from Mr. Smythies, altering the numbers of his equations into the letters *f* and ψ .

"The second differentials in (*f*) denote the whole forces, central and centrifugal; and as the central force is a function of the distances only dependent on the positions not on the velocities of the points, the assumption that the velocities vanish destroys the centrifugal forces but does not alter the central. The result of that assumption, then, gives the ratios of the central forces when the particles begin to move from a state of rest when no centrifugal forces exist. But the ratios of the central forces in

* Mr. Smythies ought to have expressed the supposition of *equal* particles, since he uses no ratio of masses different from unity.

that case are their ratios also when the particles move with any velocities, for otherwise they would be functions of the velocities and not of the positions only. The last equation (ψ) therefore gives the necessary relation between the distances and central forces of five moving particles."

On this we make no comment, nor on the conclusion that "when the number of spheres exceeds ten the number of equations is more than sufficient to determine the law of force, and no law of force is possible but that which varies directly as the distance." We shall merely remark, in justification of this notice, that a writer who is capable of the independent deduction of the relation existing among the distances of five points in space, is one whose errors are worth recording.

XXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 230.]

March 14, 1850. **O**N so-called Chylous Urine. By H. Bence Jones, M.D., A.M., F.R.S. &c.

The definition given of chylous urine is, that it is urine which is white from the suspension of fatty matter in it. An opportunity of observing a case of this disease having occurred to the author, he was led to make the experiments described in this paper. A harness-maker, age 32, half-caste, who had lived in London for twelve years, had been passing such water for nine months. On examination of the water made at 2 P.M. it solidified, looking like blanc-mange in ten minutes. It was very feebly acid, contained fibrine, albumen, blood-globules and fat; specific gravity = 1015. 1000 grs. of this urine gave—

44·42 grs. total solid residue.

8·01 grs. total ash.

14·03 grs. albumen.

8·37 grs. fat.

13·26 grs. urea and extractive matter.

·75 gr. loss.

955·58 grs. water.

In order to watch the variations produced by food and exercise in the appearance of the urine, every time the urine was made, for five days and nights it was passed into bottles marked with the hour. From these observations, and more particularly from the third, fourth, and sixth days, it was evident that the fibrine and albumen appear in the urine when no fat is there, and that the albuminous urine occurs before food has been taken, and disappears during the night with perfect rest. Thus the fourth day, at 7^h 15^m A.M., on first getting up the urine contained the slightest trace of albumen. The specific gravity = 1027; the precipitate by alcohol = 0·8 gr. per 1000 grs. urine.

At 9^h 50^m A.M., just *before* breakfast, the urine formed a solid coagulum free from fatty matter, but contained a visible deposit of

blood. Specific gravity = 1015·6; the precipitate by alcohol = 14·1 grs. per 1000 grs. of urine.

At 11 A.M., the urine was chylous or white from fatty matter.

Further experiments on the influence of rest and motion in lessening or increasing the albumen in the urine previous to food are then given.

On five different mornings, by rising early or late, and by collecting the precipitate from the urine by alcohol, the influence of rest and motion was determined. The author states that he could fix beforehand whether the urine should be albuminous or not, by directing the patient to get up, or to lie still.

The patient was bled and the serum was opalescent, but did not clear with æther: the blood contained no excess of fat. 1000 parts of blood gave—

2·63 grs. fibrine.
159·3 grs. blood-globules.
78·1 grs. solids of serum.
240·03 grs. total residue.
759·97 grs. water.

The urine made the same day was examined at different hours; that made immediately before the bleeding was quite white, and that made an hour and a half afterwards was very milky also. Specific gravity = 1018. 1000 grs. of urine gave—

56·87 grs. total residue.
10·80 grs. total ash.
13·95 grs. albumen.
7·46 grs. fat.
24·06 grs. urea, &c.
·60 gr. loss.
943·13 grs. water.

The conclusions from these experiments are,—

1. That so-called chylous urine, besides fat, may contain albumen, fibrine, and healthy blood-globules.

2. That, although the fat passes off in the urine after food is taken, yet the albumen, fibrine and blood-globules are thrown out before any food has been taken. During perfect rest the albumen ceases to be excreted; and it does not appear in quantity in the urine even after food is taken, provided there is perfect rest. A short time after rising early the urine may coagulate spontaneously, although no fat is present; and this may happen previous to food, when the urine is free from fat.

3. Though the urine made just before and a short time after bleeding was as milky as it usually was at that hour of the day, yet the serum of the blood was not milky: it did not contain a larger quantity of fat than healthy blood does.

The general results are,—

1. That the most important changes in the urine in this disease take place independently of the influence of digestion.

2. That the urine in one respect only resembles chyle, and that

is in containing, after digestion, a large quantity of fat in a very fine state of division. The supposition that the disease consists in an accumulation of fat in the blood, which is thrown out by the kidneys, carrying with it albumen, fibrine, blood-globules and salts, is altogether disproved, both by actual analyses of the blood, and by the frequent occurrence of a jelly-like coagulum in the urine when no white fatty matter can be seen to be present.

3. The disease consists in some change in the kidney by which fibrine, albumen, blood-globules and salts are allowed to pass out, whenever the circulation through the kidney is increased; and if at the same time fat is present in the blood, it escapes also into the urine. That this change of structure is not visible to the naked eye on post-mortem examination, Dr. Prout long since demonstrated; and in a case of this disease which was in St. George's Hospital, and was examined at Plymouth, no disease of the kidney was observed. From the total absence of fibrinous casts of the tubes from the urine, it is not improbable that by the microscope a difference may be detected in the structure of the mammary processes, rather than in that of the cortical part of the kidneys.

April 25.—“On the Temperature of Steam and its corresponding Pressure.” By John Curr, Esq. Communicated by J. Scott Russell, Esq., F.R.S.

The author states that it is intended in this paper to propose a simple law to determine the pressure of steam corresponding to any given temperature, *irrespectively of experiment*, taking as the sole datum, that the vaporizing point of water under a given pressure is 100 degrees, that number being taken to correspond with the scale of Celsius; also to construct formulæ in accordance therewith; and afterwards to compare their results with the actual experiments of the Academy of Sciences of Paris. He further states that the rationale of the subsequent formulæ is expressed as follows. Let it be conceived that a *given* quantity of water is vaporized under the condition that the pressure thereon is increased in the same ratio that the volume is increased, or that at intervals of temperature 1, 2, 3, &c. the volume is increased the same or in equal proportions; the temperature of the volume will be increased exactly as the square of the temperature indicated by the thermometer, supposing the instrument to be a true measure of temperature, and as the square of the volume; and the same of the pressure.

Steam being generated from an *indefinite* quantity of water and confined within a limited space, as in the usual boiler, he considers the foregoing case is reversed; for the volume being constant, the action of the fire is entirely exerted in producing increased elastic tension of the vapour; therefore the temperature of the steam at the interval 1 to 2 degrees is increased inversely in the duplicate ratio of the ratio in the case first described; that is, the pressure is increased directly as the square of the square, or fourth power of the temperature; whence the following law. The pressure of steam generated in the usual steam-boiler is directly proportional to the fourth power of its temperature, when measured by a true scale.

It being assumed that 100 degrees is the temperature of steam when its pressure is in equilibrium with a column of 30 inches of mercury, or with the pressure of one atmosphere, then F being the pressure in atmospheres, at any temperature t ,

$$F = \left(\frac{t}{100} \right)^4.$$

A comparison is instituted between theoretic experiments of the Academy of Sciences and the results of this formula, from which it appears that the temperatures deduced from the formula are invariably in defect, the greatest difference being 3.51, and the least 2.02.

June 20.—“Observations on the Nebulæ.” By the Earl of Rosse, Pres.R.S., &c. &c.

The object of this paper is to lay before the Royal Society an account of the progress which has been made up to the present time in the re-examination of Sir John Herschel's Catalogue of Nebulæ published in the *Phil. Trans.* for 1833.

Before describing any of the interesting objects the peculiar features of which the extraordinary powers of the telescope employed for their examination has brought to our knowledge, the author enters upon some details concerning the instrument itself. This telescope, which for apertures and the consequent power it possesses for the examination of faint details must for a considerable time, at least, remain unrivalled, has a clear aperture of 6 feet, with a focal length of 53 feet. It has hitherto been used as a Newtonian, but by the easy application of a little additional apparatus it may be conveniently worked as a Herschelian; and the author thinks it not improbable that, in the further examination of the objects of most promise with the full light of the speculum *undiminished by a second reflexion*, some additional features of interest will come out.

The tube reposes at its lower end upon a very massive universal joint of cast-iron, resting upon a pier of stonework buried in the ground, and it is counterpoised so that it can be moved in polar distance with great facility. The extreme range of the tube in right ascension at the equator is one hour, but greater as the polar distance diminishes. By a little subsidiary apparatus the movement of the telescope can be rendered almost exactly equatorial; but up to the present time this apparatus has not been used, as without it, the movement was found to be sufficiently equatorial for such measurements as have been required. The whole mounting was planned especially with a view of carrying on a regular system of sweeping, but as yet the discovery of new nebulæ has formed no part of the systematic work of the observatory, the known objects which require examination being so numerous that hitherto the observers have been fully occupied with them.

A clock movement was part of the original design, but as yet the telescope is not provided with one, and the want of it has not been very much felt.

Various micrometers have been tried, but, upon the whole, the

common wire micrometer with thick lines has been found to succeed the best ; for the faint details of the nebulæ are extinguished by any micrometrical contrivance which either diminishes the light of the telescope or renders the field less dark ; and thick lines have been found to be visible without illumination in the darkest night.

The telescope has two specula, one about three and a half, and the other rather more than four tons weight. Each is provided with a system of levers to afford it an equable support. Upon this system it was placed before it was ground, and has rested upon it ever since. The systems of levers with the mode of applying them in the support of the speculum are described in the paper, and also the precautions taken to guard against strain and consequent flexure of the metal. Notwithstanding these precautions, undoubted evidences of flexure in the speculum have occasionally shown themselves. It has not, however, been found that flexure, even to the extent of materially disfiguring the image of a large star, interferes much with the action of the speculum on the faint details of nebulæ, although it greatly lessens its power in bringing out minute points of light, and in showing resolvability where, under favourable circumstances, resolution had been previously effected.

It is stated that, in the spring of 1848, the heavier of the two specula, for nearly three months, performed admirably, very rarely exhibiting the slightest indication of flexure. It then remained inactive for some time before and after the solstice, and when observations with it were again commenced, it was found to be in a state of strain. On cautiously raising it a little by screws, for the purpose of readjusting the levers, it was found that the unequal strain of the screws had produced permanent flexure, so that the speculum did not again perform well until after it had been reground. Recently an alteration has been made in the mode of supporting the lighter of the two specula, which now rolls freely on eighty-one brass balls that support it nearly equably. After referring to other causes of unequal action, among which the varying state of the atmosphere is one of the most serious, the author remarks that the Society will not be surprised should it be in his power at a future time to communicate some additional particulars, even as to the nebulæ which have been most frequently observed.

The very beautiful sketches which illustrate the paper, are, it is remarked, on a very small scale, but are sufficient to convey a pretty accurate idea of the peculiarities of structure which have gradually become known. In many of the nebulæ they are very remarkable, and seem even to indicate the presence of dynamical laws we may perhaps fancy to be almost within our grasp.

On examining these sketches it will at once be remarked, as stated by the author, that the spiral arrangement so strongly developed in H. 1622, 51 Mesier, is traceable more or less distinctly in several of the sketches. More frequently indeed there is a nearer approach to a kind of irregular interrupted annular disposition of the luminous material, than to the regularity so striking in 51 Mesier ; but it can scarcely be doubted that these nebulæ are systems of a very similar

nature, seen more or less perfectly, and variously placed with reference to the line of sight. The author adverts to the description of this nebula by Mesier, Sir William Herschel and Sir John Herschel, and remarks, that taking the figure given by Sir John, and placing it as it would be seen with a Newtonian telescope, we shall at once recognize the bright convolutions of the spiral which were seen by him as a divided ring: thus with each increase of optical power the structure has become more complicated, and more unlike any thing which we could picture to ourselves as the result of any form of dynamical law of which we find a counterpart in our system. After pointing out the importance of measurements and the difficulty of taking them satisfactorily, the author states, that of a few of the stars with which the nebula is pretty well studded, measurements with reference to the principal nucleus were taken by his assistant Mr. Stoney in the spring of 1849, and that these have been repeated this year during the months of April and May, and also some measures taken from the centre of the principal nucleus to the apparent boundary of the spiral coils in different angles of position. A hope is then expressed that, as several of these stars are no doubt within reach of the great instruments at Pulkova and at Cambridge, U.S., the distinguished astronomers who have charge of them will consider the subject worthy of their attention.

The spiral arrangement of 51 Mesier was detected in the spring of 1845, and in the following spring an arrangement, also spiral, but of a different character, was detected in 99 Mesier. The author considers that 3239 and 2370 of Herschel's 'Southern Catalogue' are very probably objects of a similar character; and as the same instrument does not appear to have revealed any trace of the form of 99 Mesier, he does not doubt that they are much more conspicuous, and therefore entertains the hope that, whenever the southern hemisphere shall be re-examined with instruments of great power, these two remarkable nebulae will yield some interesting result.

The author briefly refers to the other spiral nebulae discovered up to the present time, which are more difficult to be seen, and to clusters in the exterior stars of which there appears to be a tendency to an arrangement in curved branches. He then passes to the regular cumular nebulae, in which, although they are perceived at once to be objects of a very different character, there still seems to be something like a connecting link.

Among the nebulous stars two objects are stated to be well worthy of especial notice—No. 450 of Sir John Herschel's Catalogue, and ι Orionis. A representation of No. 450, as seen with the six feet telescope, is given. It has been several times examined, but as yet not the slightest indication of resolvability has been seen. The annular form of this object was detected by Mr. Stoney when observing alone, but Lord Rosse has since had ample opportunities of satisfying himself that the object has been accurately represented. A representation of ι Orionis is likewise given. The remarkable feature in this object, the dark cavity not symmetrical with the star, was also discovered by Mr. Stoney when observing alone with the

three feet telescope. Lord Rosse has since seen it several times and sketched it. A small double star *n, f i* has similar openings, but are not so easily seen. These openings appear to be of the same character as the opening within the bright stars of the trapezium of Orion, the stars being at the edges of the opening. Had the stars been situated altogether within the openings, the suspicion that the nebula had been absorbed by the stars would perhaps have suggested itself more strongly. As it is, the author thinks we can hardly fail to conclude that the nebula is in some way connected with these bright stars, in fact that they are equidistant, and therefore, if the inquiries concerning parallax should result in giving us the distances of these bright stars, we shall have the distance of this nebula.

The long elliptic or lenticular nebulae are stated to be very numerous, and three sketches of remarkable objects of this class are given.

In proceeding with the re-examination of Sir John Herschel's Catalogue, several groups of nebulae have been discovered, in some of which nebulous connexion has been detected between individuals of the group, in others not. Sketches of some have been made and measures taken; but although the subject of grouped or knotted nebulae is considered one of deep interest, it has not yet been proceeded with far enough to warrant entering upon it in the present paper.

The conclusion of the paper is occupied with remarks relating to each figure, in order to render the information conveyed by it more complete, and these are stated to be for the most part extracts selected from the Journal of Observations.

XXXIX. *Intelligence and Miscellaneous Articles.*

TENACITY OF METALS.

AS the results of numerous experiments, M. Baudrimont has arrived at the following conclusions:—

1. That the tenacity of metals varies with their temperature.
2. That it generally decreases, though not without exception, as the temperature rises.
3. That with silver the tenacity diminishes more rapidly than the temperature.
4. That with copper, gold, platina and palladium, it decreases less rapidly than the temperature.
5. That iron presents a very peculiar and remarkable case: at 212° F. its tenacity is less than at 32°; but at 392° its tenacity is greater than at 32°.—*Comptes Rendus*, Juillet 29, 1850.

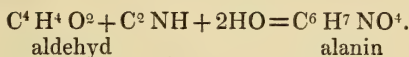
ON THE ARTIFICIAL FORMATION OF LACTIC ACID AND ALANIN.

M. A. Strecker states that lactic acid, when treated with binocide of lead, yields carbonic acid and aldehyd; on the other hand, it is separated by heat into oxide of carbon, aldehyd and water, as

shown by M. Engelhardt. These reactions induced the author to suppose that lactic acid might be a compound constituted of formic acid and hydruret of benzoyl (aldehyde benzoïque). Guided by these ideas, M. Strecker succeeded in forming lactic acid by means of aldehyd and hydrocyanic acid, which is readily converted into lactic acid.

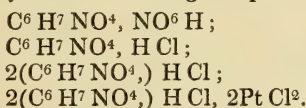
The following are the results of M. Strecker's experiments :—

Aldehyd, ammonia and prussic acid, treated in aqueous solution by hydrochloric acid, combine and fix two equivalents of water : there are formed sal-ammoniac and the hydrochloric combination of a new substance homologous with glycocoll and leucin, which the author has named *alanin*. The following equation represents the formation of alanin :—



Alanin is isomeric with lactamide, urethran and sarcosin ; it differs from these compounds by its properties. Alanin crystallizes in oblique rhombic prisms ; it dissolves readily in water, but is insoluble in alcohol or in æther. The solution of alanin has a distinct sugary taste ; it has no action on litmus paper. Alanin exposed to a moderate heat undergoes no change ; it requires a temperature exceeding 392° F. to sublime it, and, this is effected without changing its composition.

Alanin combines with acids, and gives a double salt with chloride of platina ; these combinations, the composition of which does not differ from the salts formed with organic bases, possess an acid reaction. They are readily soluble in water and in alcohol. M. Strecker has analysed the following compounds :—



Alanin combines also with metallic oxides, forming compounds soluble in water, and less soluble in alcohol. In these combinations the metallic oxide replaces 1 equivalent of the water of the alanin. The author has analysed the copper salt, crystallized in prisms of a fine blue colour ($\text{C}^6 \text{H}^6 \text{NO}^3, \text{CuO} + \text{HO}$), and which loses 1 equivalent of water at 248° F. ; the silver salt ($\text{C}^6 \text{H}^6 \text{NO}^3, \text{AgO}$), and the lead salt ($\text{C}^6 \text{H}^6 \text{NO}^3, \text{PbO}, + \text{PbO}, \text{HO}$).

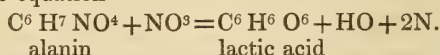
Alanin combines also with nitrate of silver. This compound, represented by $\text{C}^6 \text{H}^7 \text{NO}^4, \text{AgO}, \text{NO}^5$, crystallizes in colourless rhombic tables ; it is soluble in alcohol.

It will be observed that the properties of alanin differ much from those of urethan and lactamide, with which it is isomeric ; it more nearly resembles sarcosin, from which however it is distinguished by its property of combining with metallic oxides. It is therefore alanin, and not sarcosin, which is the homologue of glycocoll and leucin. By substituting valeral for aldehyd, the author hopes to obtain leucin.

Alanin is not acted upon by acids, nor by a boiling concentrated solution of potash. When fused with hydrate of potash, hydrogen is evolved, and there are formed hydrocyanic and acetic acids, which remain combined with the potash.

If nitrous gas (NO^3) be made to act upon a solution of alanin, much nitrogen is evolved: the solution evaporated with a gentle heat gives a syrupy residue, which, treated with æther, yields an acid which was readily recognized by its reactions, and by the elementary analysis of its zinc salt, to be lactic acid. In fact the analysis of this salt led to the formula $\text{C}^6 \text{H}^5 \text{O}^3$, $\text{ZnO} + 3\text{Aq}$. At 212°F . it loses 3 equivalents of water. It is therefore common lactic acid, and not that found in muscular flesh.

The formation of lactic acid in the reaction described is represented by the equation



This reaction is interesting, especially when it is considered that the lactic acid, the formula for which ought probably to be doubled, is derived from grape-sugar by a simple molecular modification.—*Comptes Rendus*, Août 1850.

ON THE ACTION OF BASES UPON SALTS.

BY M. ALVARO REYNOSO.

It is generally admitted that when a salt, the oxide of which is insoluble, is treated with an alkaline solution, the oxide is precipitated without redissolving, unless in its uncombined state it is soluble in an excess of the alkali with which the salt containing it is mixed.

On investigating the action of potash and soda on the arsenites, the author observed some facts, which, if they be not precisely contrary to the general law which he has cited, prove at least that this phenomenon of precipitation is sometimes intimately connected with the nature of the salt above the precipitate; so that, in certain cases, this salt may determine the solubility of the oxide.

Thus, for example, the oxides of copper, uranium, cobalt, nickel, silver, mercury and sesquioxide of iron, being insoluble in potash and in soda, when these alkalies are poured into the arsenites of these bases, precipitation of the insoluble oxide ought merely to occur, with the formation of arsenite of potash or soda, unaccompanied with any action on the oxide; M. Reynoso has, however, found that the arsenites of all these oxides are completely soluble in potash, although in a separate state they are insoluble.

Arsenite of iron is very soluble in potash; the solution of arsenite of copper is blue, and after a certain time it decomposes into protoxide of copper, which precipitates, whilst the arsenite of potash becomes arseniate.

The decomposition of the solution of arsenite of mercury is almost instantaneous. The solution of silver is colourless, and decomposes very slowly, precipitating silver as a black powder. This solution

does not precipitate with chloride of sodium ; on the contrary, chloride of silver, which is insoluble in potash, dissolves in it very readily when arsenite of potash is added.

M. Reynoso took advantage of these two properties of arsenite of silver to effect the reduction of the salts of palladium by means of silver. The experiment is performed as follows : chloride of palladium, to which arsenite of potash is previously added, is poured into a solution of arsenite of silver in potash. A black powder is readily precipitated, which contains metallic silver and palladium. Chloride of platina is much more readily reduced than that of palladium. It is to be observed that, in these reactions, arsenite of silver decomposes much more quickly than when it is alone.

The arsenites of cobalt, nickel and uranium, dissolve completely in potash and soda, only in the nascent state. For this purpose it is necessary to employ arsenite of potash with a great excess of potash, and to pour this solution into a soluble salt of cobalt, nickel or uranium.

These reactions will be readily understood, by admitting that arsenite of potash is capable of forming a double soluble salt with the compound of potash and these oxides, and that it is under this influence that their solution is effected. When potash is made to act upon an insoluble salt, the oxide of which is itself soluble in an excess of potash, a solution can only occur on the condition of the formation of a soluble double salt. Thus, for example, the author has stated that arsenite of lead is insoluble in potash. The proof that these reactions depend upon the nature of the salt formed is, that arsenite of lead, which is insoluble in potash, is completely soluble in soda.

When potash is added to an insoluble salt, it first combines with the acid, and the oxide set free will remain without acting upon the salt formed ; but if excess of potash be added, and the oxide is soluble in it, then if the compound of this oxide with potash cannot unite with the supernatant salt, two soluble salts will be present, which, being able to form an insoluble salt by their decomposition, will regenerate the primary salts. This, however, is a very rare case ; for experiment has proved that almost all the salts of potash have the property of forming a soluble double salt with the oxides soluble in potash.

With respect to ammonia, the author has stated the solubility therein of arsenite of sesquioxide of iron.

In concluding, M. Reynoso observes that four cases of the action of potash on insoluble salts may occur :—

1. In the case of certain oxides, which, when uncombined, are soluble in potash, and which form soluble double salts with all the salts of potash, the solution may be observed under all circumstances.

2. In some cases, uncombined oxides, which are soluble in potash, will form salts insoluble in potash when the acid is not such as yields a soluble double salt with the compound of the oxide and potash.

3. Some other uncombined oxides, insoluble in potash, may nevertheless sometimes form a soluble double salt, and they will con-

the reaction of ammonia on the chlorides and bromides of the radicals of the alcohols. The following is a method of preparing ethyalmin, which probably possesses some advantages.

If the vapour of anhydrous sulphuric acid be absorbed by common æther, sulphuric æther, properly so called, or sulphatic æther (C^4H^5O , SO^3) is formed, which, when water is added to it, remains dissolved in the excess of æther, from which it may be separated by spontaneous evaporation.

Sulphatic æther, treated with ammonia, acts like an anhydrous acid; it absorbs this base, and forms an ammoniacal salt of an amided acid. This new salt is represented by the formula $4SO^3$, $C^{16}H^{23}NO^4 + NH^3$; 4 equivalents of the compound æther have absorbed 2 equivalents of ammonia, one of which has entered into the composition of the acid. On treating this salt with carbonate of barytes or of lead, ammonia is evolved, and barytic or lead salts are formed with the new acid, named by M. Strecker *ethamic acid*. This acid, treated with a hot solution of potash, yields ethyalmin, as proved by the analysis of the platina salt, which gave as its composition C^4H^7N , HCl , $PtCl^2$; there are also formed alcohol and sulphuric acid.—*Comptes Rendus*, Août 1850.

ON THE ACTION OF CARBON ON METALLIC SOLUTIONS.

BY M. ESPRIT.

It is stated by the author, in reference to the experiments on the above subject by M. Schönbein, that long since MM. Chevallier, Girardin, Graham and Weppen, had noticed some of the interesting phenomena produced by it. But all these chemists made their experiments with perfectly purified animal charcoal; M. Schönbein, on the contrary, made use of ivory-black and coke, that is to say, of two varieties of carbon of complex constitution, and but little favourable for studying the action peculiar to carbon and for distinguishing it from that attributable to the foreign substances which accompany it. Ivory black, according to the analysis of M. Braconnot, contains only 79 per cent. of carbon, the remaining 21 parts being composed of resinous matters, sulphate and phosphate of ammonia, chlorides, &c., all of which are substances which can and must ever possess a decided influence on the results of the experiment. It is indeed true that the greater part of these impurities may be got rid of; and it would be satisfactory to know that the precaution had been taken, but it is not mentioned that it was so. The same may be said of coke, which always contains, according to the manner in which it has been prepared, variable proportions of sulphur and of sulphurets, of which it is requisite to take notice.

M. Esprit is of opinion that the reduction of metallic solutions by carbon does not always occur; and he differs from M. Schönbein in supposing that bichloride of mercury is by its action reduced to protochloride. It is quite true that when a cold solution, even of bichloride of mercury, is treated with powdered charcoal, no trace of

the bichloride is to be found in the filtered liquor: and at first it would be quite natural to suppose that reduction had occurred, and that protochloride of mercury would be found insoluble in the filter; it is however readily shown that this is not the case, as was proved, according to M. Esprit, by the following experiment:—a solution was made of 1 grm. of sublimate in 100 grms. of distilled water, and this solution was treated with 20 grms. of well-washed animal charcoal: the liquor, treated with potash, hydrosulphate of ammonia and iodide of potassium, did not indicate the presence of a mercurial salt; but on washing the charcoal which had been used in the experiment with a mixture of alcohol and æther, sublimate was rapidly dissolved; and in so large quantity, that when a tube was dipped into the solution, it gave a very distinct red precipitate with a solution of iodide of potassium.

This experiment, which the author repeated several times, and always with the same success, induced him to think that carbon does not act upon metallic solutions merely as a reducing agent, nor does he attribute it to the mere porosity of the charcoal, but he supposes that the metallic salt is retained by a peculiar force or special affinity. In studying the action of charcoal on metallic solutions, it is requisite to employ it quite free from sulphurets and calcareous salts, the presence of which complicates the operation, and does not allow of a proper estimate of the peculiar action of the charcoal.—*Journ. de Chim. Méd.*, Septembre 1850.

ON THE COPPER TEST FOR SUGAR. BY M. LASSAIGNE.

It has been long known, according to the experiments of M. Frommherz, that tartrate of copper dissolved in a solution of potash is easily reduced when heated with glucose, and converted into sub-oxide of copper. The same author has stated that cane-sugar, which differs in composition from glucose, does not act upon this reagent. It is on these facts that M. Barreswil has founded his process for estimating the quantity of sugar.

The employment of this reagent is even indicated, in several recent chemical works, as capable of distinguishing between cane-sugar and glucose.

The alkaline solution of copper, employed as a test liquor, is prepared by two published methods; one by M. Barreswil, and the other by M. Poggiale. The first consists in dissolving with heat, in one-third of a litre of distilled water, 50 grammes of bitartrate of potash, and 40 grammes of carbonate of soda, and afterwards adding 30 grammes of powdered crystallized sulphate of copper; after boiling, allow the solution to cool, and lastly add 40 grammes of potash dissolved in one-fourth of a litre of water; it is made up a litre and again boiled. The second method, proposed by M. Poggiale to determine the presence of sugar of milk, which also reduces the oxide of copper, like glucose, consists in dissolving in 200 grammes of water 10 grammes of crystalline sulphate of copper, 10 grammes of

bitartrate of potash, and 30 grammes of potash. This solution when filtered is of an intense blue colour, and ought to be kept in the dark.

The trials made with this solution proved that cane-sugar or beet-sugar may produce the same reaction as glucose on the alkaline solution of oxide of copper, according to the conditions of operating and the modifications which this kind of sugar may undergo by the influence of heat alone, or with water.

M. Lassaigne thinks he has ascertained that glucose acts more readily, and at a lower temperature, on the copper reagent than common sugar does; but the latter, if heated till it begins to assume an amber colour of less or greater depth, then acts like the former. In fact, an aqueous solution of glucose artificially prepared with starch, and containing one-fiftieth of its weight of this sugar, treated with the copper test at a heat of 64° to 68° F., reacts in less than three or four minutes, or by holding the tube inclosed in the palm of the hand. Under similar conditions cane-sugar produces no effect on the solution of copper; but it may be kept for some time near 212° without giving any appearance of reduction. By prolonging the time of boiling, action is at first perceived by change in the blue colour of the solution, slight turbidness, and the separation of a pulverulent yellow precipitate, which eventually becomes brick-red.

The action of cane-sugar, not modified by heat, is slow upon the alkaline solution of copper; whereas when it has been subjected to a more or less high temperature, the action is often as prompt as that of glucose on the same reagent.

Heat, by its action on cane-sugar, ought naturally to put us on our guard in employing the solution of copper above mentioned for determining glucose in manufactured products in which it is supposed to exist. The employment of this reagent is to be suspected of inducing error in certain cases; thus barley-sugar and *pate de gomme*, prepared with cane-sugar slightly *caramelized* by heating the substances which enter into their composition, acted as readily on the test as pure glucose.

It has also been proved that potash, which acts in so characteristic a manner upon glucose, and which has been proposed for distinguishing this kind of sugar from cane or beet-sugar, often gives with these latter sugars reactions analogous to those developed by glucose. In the examination of sugars, and various alimentary or medicinal preparations into which they enter, the influence which heat may produce on these products, and the modifications which result from it, should be considered.—*Journ. de Chim. Méd.*, Juillet 1850.

NEW REAGENT FOR OXIDE OF CARBON.

MM. Stas, Dogere and Felix Leblanc, wishing to determine the oxygen in carburetted hydrogen by ammoniacal protochloride of copper, found that this reagent dissolved a great quantity of oxide of carbon and even of olefiant gas. M. Leblanc, having undertaken the investigation of this property, obtained the following results:—

1. In passing a current of oxide of carbon into a solution of protochloride of copper in hydrochloric acid, the gas was absorbed in considerable quantity, and with a rapidity comparable to that with which carbonic acid is absorbed by potash; but the temperature was comparatively but slightly raised.

2. The ammoniacal protochloride of copper, out of the contact of the air, acts in the same manner, and the quantity of gas absorbed is the same for the same quantity of copper dissolved. This solution becomes blue afterwards by exposure to the air, and may again serve to absorb oxygen.

3. The acid protochloride of copper, saturated with oxide of carbon, may be diluted even with a large quantity of water without precipitating protochloride of copper, as before absorption, and without any disengagement of gas. The addition of alcohol does not render it turbid. Æther appears to destroy, at least partially, the compound which M. Leblanc has not hitherto been able to isolate. Ebullition or a perfect vacuum expels the gas.

4. The fact of the absorption of oxide of carbon by the protochloride of copper appears to be of the same order as the absorption of nitric oxide by the salts of protoxide of iron, inasmuch as the absorption appears to occur in definite proportions. The numbers approximate equal equivalents of copper and oxide of carbon.

5. The protosalts of iron or tin do not act upon oxide of carbon.

6. The various salts of protoxide [suboxide?] of copper dissolved in ammonia absorb oxide of carbon like the protochloride of copper.

7. Cyanogen is also absorbed by protochloride of copper; there is then formed a deposit of a chrome-yellow colour, which is rapidly modified in the air.—*Journ. de Chim. Méd.*, Juillet 1850.

ON A CAUSE OF VARIATION IN THE ANGLES OF CRYSTALS.

BY M. J. NICKLÈS.

The author states that the cause is the intervention of foreign substances. In September 1849 he pointed out this cause in the variations of the angles of the prisms of sugar of gelatine; and he now adduces a new fact which readily allows of verifying the influence that a small quantity of foreign substances may exert on the crystalline form of bodies which are deposited in its presence. When a solution of chloride of cobalt containing an excess of sal-ammoniac is allowed to evaporate spontaneously, crystals of the last-mentioned salt are obtained which are more or less coloured, the angles of which are always near, but never 90° : the difference often exceeds 7° , and yet these crystals contain only 0.5 to 1 per cent. of chloride of cobalt. The same fact has been observed with respect to crystals of hydrochlorate of ammonia deposited in the presence of bichloride of platina, chloride of nickel, and also with chloride of potassium deposited under similar circumstances.—*L'Institut*, No. 852.

ON THE EXTRACTION OF IODINE FROM PLANTS AND FROM COAL. BY M. BUSSY.

Some doubts having been expressed by several persons in the Academy with respect to the accuracy of the statements respecting the existence of iodine in certain plants, the author deposited a specimen of cress and of the iodine which he had obtained from it, and also of iodide of potassium procured from the same plant.

M. Bussy also remarks that in 1839 he showed that the coal of Commentry contains iodine. Some portions of this coal contain much sulphuret of iron; whence it happens that whilst working, masses often undergo a kind of slow combustion. The heat thus produced gives rise to thick vapours which condense on the surface, and the product is found to consist of sulphuret and other arsenical compounds, with much sal-ammoniac, containing hydriodate of ammonia: at the period mentioned the author merely stated the peculiar reactions of iodine, without trying to isolate it. Not having any of the natural product in his possession in which he had first met with iodine, he had recourse to the products of the distillation of coal for obtaining gas; and he found in the ammoniacal liquor a considerable quantity of iodine, and such as he could separate and estimate.

The process which he adopted was to add to a certain quantity of the condensed water enough potash to convert the hydriodate of ammonia into iodide of potassium; by evaporation to dryness and calcination, the tarry matter was destroyed; and the residue being treated with alcohol, yielded iodide of potassium. The iodine was estimated by means of iodide of palladium, by the decomposition of which by heat iodine was obtained, of which a specimen was presented to the Academy.

Three kilogrammes of the condensed water of the establishment at the *barrière* of Fontainebleau yielded 0.59 gr. of iodine, nearly 0.2 per kilogramme, or 2 ten-thousandths: iodine was also found in the liquor of another establishment, which renders it probable that it will be found in all varieties of coal.

M. Bussy remarks that the quantity stated does not include the whole of the iodine contained in the coal, since a quantity remains in the coke, which may be obtained by incineration.

M. Bussy observes, that the distilled product of gas-works may possibly be employed for the æconomical preparation of iodine, especially if it could be obtained without prejudice to the separation of the ammoniacal salts.—*L'Institut*, No. 853.

BROMINE A PRODUCT OF THE DISTILLATION OF COAL.

BY M. MENE.

The author, who is chemical assistant at the College of France, states that he has discovered bromine in the ammoniacal liquor obtained as above mentioned. He has also found the iodine previously mentioned by M. Bussy.—*Ibid.* No. 854.

DECOMPOSITION OF METALLIC ACIDS BY IODIDE OF POTASSIUM.

BY M. SCHÖNBEIN.

The author states that the antimonie, chromic, molybdic, tungstic, stannic, titanie, arsenic and even phosphoric acids, are decomposed either with or without heat by iodide of potassium. The iodine is disengaged, and a salt of potash is formed. Perchloride of iron, peroxide of iron, the persalts of iron and the salts of copper, act similarly on iodide of potassium.

Bromide of potassium acts like the iodide; but neither the chloride of potassium or sodium is decomposed under the same circumstances: the chlorides of barium, strontium, calcium and magnesium, and probably those of many other metals, lose chlorine when heated with bichromate of potash.

M. Schönbein states also several experiments performed to show the deoxidizing power of powdered charcoal in different solutions.

A solution of sesquichloride of iron is reduced to that of protochloride when agitated with powdered charcoal. Calcined lamp-black is more powerful than common wood-charcoal: even coke produces the effect.

The solutions of persulphate, pernitate and peracetate of iron are reduced, like the perchloride, by charcoal. The red prussiate of potash dissolved in water is reduced to the state of yellow prussiate when treated with the powder of common charcoal.

Under the same circumstances the bichloride and pernitate of mercury are reduced to protosalts of mercury. These reactions are certainly very curious; but the interest is doubled by experiments undertaken to explain them. What is the effect of the charcoal on these reductions? This question is not solved by M. Schönbein. It is difficult to believe that carbonic acid is formed; and yet if calcined lamp-black is proper for these experiments, as stated by M. Schönbein, it is impossible to attribute these reductions to the hydrogen which charcoal may contain.—*Journ. de Pharm.*, Avril, 1850.

NOTE BY M. DU BOIS-REYMOND ON M. MATTEUCCI'S PAPER
ON ELECTRO-PHYSIOLOGY.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

The Philosophical Magazine for June 1850 (vol. xxxvi. p. 489) contains an extract from a paper on Electro-Physiology by Signor C. Matteucci, read at the Royal Society, in which I find the following sentence:—"M. Du Bois-Reymond (*Comptes Rendus*) has related an experiment seeming to lead to the inference that section of the spinal marrow increases the excitability of the lumbar nerves, at least during a certain period of time."

I beg to state that I never have, either in the *Comptes Rendus* or anywhere else, described such an experiment, or any similar obser-

vation. I cannot but express some surprise at being quoted by Signor Matteucci on a subject which I have not touched upon; whilst, on so many important occasions, this author seems to have been quite unacquainted with the real results I have obtained in electro-physiology.

I am, Gentlemen,

Your obedient Servant,

Berlin, 21 Carlstr.

August 25, 1850.

EMIL DU BOIS-REYMOND.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1850.

Chiswick.—August 1. Hazy. 2. Densely overcast: slight haze: clear. 3, 4. Fine. 5, 6. Very fine. 7. Very fine: rain at night. 8. Cloudy: slight rain. 9. Fine. 10. Fine: drizzly. 11. Fine. 12. Fine: thunder: clear at night. 13. Heavy clouds: very fine: lightning at night. 14. Cloudy: very fine. 15. Slight rain: cloudy: clear. 16. Very fine. 17, 18. Cloudy and fine. 19. Boisterous, with dry air: clear. 20. Fine. 21. Overcast: heavy rain: frosty at night. 22. Clear and fine. 23, 24. Cloudy: fine: clear. 25. Overcast: drizzly. 26. Slight rain. 27. Fine. 28. Very fine. 29. Clear and fine. 30. Very fine. 31. Overcast.

Mean temperature of the month	59°·38
Mean temperature of Aug. 1849	62 ·91
Mean temperature of Aug. for the last twenty-four years ...	62 ·18
Average amount of rain in Aug.	2·41 inches.

Boston.—Aug. 1, 2. Cloudy. 3. Fine. 4. Cloudy. 5—7. Fine. 8. Fine: rain with thunder and lightning P.M. 9. Cloudy: rain A.M. and P.M. 10. Cloudy. 11. Fine. 12. Fine: rain P.M. 13. Cloudy: rain A.M. and P.M. 14. Fine. 15. Cloudy. 16. Fine. 17. Cloudy. 18. Fine. 19. Cloudy: stormy. 20. Fine: stormy. 21. Fine. 22. Fine: rain P.M. 23, 24. Fine. 25. Cloudy: rain P.M. 26. Fine. 27. Cloudy: rain P.M. 28—30. Fine. 31. Cloudy.

Applegarth Manse, Dumfries-shire.—Aug. 1. Slight shower at night: fine day. 2. Slight drizzle: fine day. 3. Fair and fine, though cool. 4. Heavy rain and high wind. 5. Fine A.M.: rain P.M. 6. Fair and fine A.M.: shower P.M. 7. Fair and fine: rain P.M. 8. Rain A.M.: cleared: rain P.M. 9. Rain: cleared P.M. 10. Rain P.M. 11. Rain. 12. Rain: fine P.M. 13. Fair and fine. 14. Fair: sultry. 15. Warm: sultry. 16. Fair: slight drizzle. 17. Fine: slight drizzle. 18. Wet nearly all day. 19. Shower: stormy. 20. Showers short and frequent. 21. Fine harvest day. 22. Showery: hail: cool. 23. Fair till 5 P.M.: rain heavy. 24. Frequent showers: hail. 25. Wet day: cleared P.M. 26. Fine harvest day: slight shower. 27. Heavy rain all day: flood. 28. Fine harvest morning: one shower. 29. Fine harvest morning: fair all day. 30. Fine harvest morning. 31. Slight drizzle A.M.: cleared.

Mean temperature of the month	55°·1
Mean temperature of Aug. 1849	56 ·7
Mean temperature of Aug. for twenty-eight years	57 ·0
Rain (average) for twenty-three years in Aug.	3·60 inches.

Sandwich Manse, Orkney.—Aug. 1. Clear: cloudy. 2. Bright: cloudy. 3. Showers: cloudy. 4. Rain: clear. 5. Cloudy: clear. 6. Clear. 7. Clear: fine: cloudy. 8. Rain: thunder-showers. 9. Cloudy: rain. 10. Fog: fine. 11. Rain: cloudy. 12. Fine: hot: fine. 13. Fine. 14. Fine: cloudy: fine. 15. Damp: cloudy: fine. 16. Cloudy. 17. Bright: cloudy. 18. Showers: cloudy. 19, 20. Showers. 21. Bright: clear. 22. Bright: rain and thunder. 23, 24. Showers. 25. Rain: drizzle. 26. Showers: drizzle: showers. 27. Damp: rain. 28. Showers: rain. 29. Showers. 30. Cloudy: rain. 31. Showers.

Days of Month.	Barometer.						Thermometer.				Wind.				Rain.			
	Chiswick.		8 $\frac{1}{2}$ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8 $\frac{1}{2}$ a.m.	Dumfries-shire.		Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	
	Max.	Min.		9 a.m.	9 p.m.	9 $\frac{1}{2}$ a.m.	6 $\frac{1}{2}$ p.m.	Max.	Min.		Max.	Min.						
1850.																		
Aug.																		
1.	30.171	30.119	29.65	30.12	30.05	30.15	29.94	70	57	65	70	47	58	.01	.08		.09	
2.	30.096	30.028	29.52	29.94	29.92	29.84	29.86	69	55	68	66 $\frac{1}{2}$	48	59				.08	
3.	30.046	29.980	29.50	29.91	29.85	29.80	29.80	77	53	74	62 $\frac{1}{2}$	51 $\frac{1}{2}$	57				.27	
4.	29.941	29.812	29.26	29.53	29.50	29.45	29.40	77	52	70	62 $\frac{1}{2}$	51 $\frac{1}{2}$	54 $\frac{1}{2}$	.02				
5.	29.750	29.659	29.18	29.58	29.58	29.42	29.62	82	56	71	61	52 $\frac{1}{2}$	57					
6.	29.733	29.720	29.20	29.59	29.68	29.63	29.74	74	49	62	65	48	55 $\frac{1}{2}$					
7.	29.871	29.863	29.33	29.73	29.57	29.80	29.73	77	58	60	66 $\frac{1}{2}$	40	58	.08				
8.	29.733	29.659	29.14	29.48	29.40	29.49	29.47	75	51	72	66	55 $\frac{1}{2}$	56	.02			.07	
9.	29.740	29.700	29.06	29.30	29.43	29.38	29.43	71	51	62	66	53	60 $\frac{1}{2}$		.19		.12	
10.	29.875	29.838	29.26	29.62	29.60	29.61	29.61	75	58	64	66	54	55		.06		.44	
11.	29.823	29.722	29.22	29.52	29.48	29.48	29.65	74	50	63	66	52	56	.04			.21	
12.	29.821	29.693	29.24	29.65	29.82	29.88	29.96	71	46	63	65	53	57	.10			.05	
13.	29.901	29.855	29.35	29.85	29.83	29.91	29.89	75	47	61	69	45	59		.87			
14.	30.014	29.994	29.52	30.00	30.02	30.02	30.08	72	55	63 $\frac{1}{2}$	66 $\frac{1}{2}$	45	58		.40			
15.	30.011	29.961	29.54	30.04	30.00	30.04	30.05	70	52	63 $\frac{1}{2}$	73	46	60 $\frac{1}{2}$	.01				
16.	29.972	29.944	29.46	29.96	29.90	30.02	29.98	78	53	63	69	56	59				.06	
17.	30.003	29.966	29.40	29.87	29.90	29.96	29.96	76	49	68	64	55	55				.24	
18.	29.996	29.767	29.30	29.63	29.46	29.29	29.07	76	49	63	63	52	54 $\frac{1}{2}$				.21	
19.	29.795	29.753	29.17	29.35	29.40	28.99	29.32	68	45	60	59	48	56				.18	
20.	29.718	29.706	29.17	29.45	29.48	29.38	29.50	68	43	55	57	44	51 $\frac{1}{2}$	.01			.27	
21.	29.646	29.612	29.22	29.48	29.49	29.50	29.53	62	32	50	59	39 $\frac{1}{2}$	57 $\frac{1}{2}$	.45				
22.	29.837	29.799	29.34	29.43	29.52	29.49	29.46	66	39	50 $\frac{1}{2}$	58	36	52				.32	
23.	29.985	29.882	29.46	29.68	29.73	29.44	29.70	66	39	51 $\frac{1}{2}$	58	39	51		.13	0.25	.19	
24.	30.040	29.739	29.56	29.80	29.83	29.82	29.77	72	40	53	68	46	52	.10			.19	
25.	30.068	29.899	29.44	29.51	29.45	29.30	29.14	67	54	60	62	48 $\frac{1}{2}$	51	.10			.25	
26.	30.153	29.799	29.28	29.59	29.98	29.42	29.85	71	36	60	59 $\frac{1}{2}$	50	49		.10		.03	
27.	30.213	29.908	29.70	29.88	29.67	29.86	29.76	72	49	56	58	38	52				.42	
28.	30.114	30.064	29.56	29.89	29.94	29.73	29.68	67	40	53 $\frac{1}{2}$	56 $\frac{1}{2}$	43	50		.07		.53	
29.	30.153	30.141	29.66	29.98	30.02	29.87	30.02	67	32	52	57 $\frac{1}{2}$	45	49				.20	
30.	30.253	30.221	29.80	30.10	30.13	30.05	30.04	68	33	50	57 $\frac{1}{2}$	35	53				.19	
31.	30.300	30.292	29.83	30.10	30.12	30.01	30.11	62	42	52	61 $\frac{1}{2}$	49	55					
Mean.	29.960	29.873	29.39	29.747	29.763	29.678	29.713	71.51	47.26	60.3	62.9	47.5	55.20	0.97	1.90	3.91	4.61	

THE
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[THIRD SERIES.]

NOVEMBER 1850.

XL. *Remarks on an alleged proof of the "Method of Least Squares," contained in a late Number of the Edinburgh Review. In a Letter addressed to Professor J. D. Forbes, by R. L. ELLIS, Esq., late Fellow of Trinity College, Cambridge*.*

MY DEAR SIR,

THE review of Quetelet's "*Lettres à S. A. R. le Duc régnant de Saxe Cobourg et Gotha*," which appeared in the July Number of the *Edinburgh Review*, contains a new demonstration of the method of least squares which ought not, I think, to pass unnoticed. If it is correct, it is so much simpler than those which have hitherto been received, that it ought to supersede them; and if not, the sooner its incorrectness is pointed out the better.

Some years since, in a paper published in the *Cambridge Transactions* for 1844, I made an analysis of all the demonstrations, or professed demonstrations of the method of least squares, with which I was then acquainted, and I therefore read this new one with more attention than you perhaps have given to it.

The reviewer gives some account of the history of the subject, and remarks that the demonstration of the least squares was first attempted by Gauss, but that his proof is no proof at all, because it assumes that in the case of a single element the arithmetical mean of the observed values is in all cases the most probable value, "a thing to be demonstrated, not assumed." Gauss afterwards gave another demonstration, which is perfectly rigorous; but of this the reviewer takes no notice, though it is mentioned in at least one of the works on the

* Communicated by the Author.

Phil. Mag. S. 3. Vol. 37. No. 251. Nov. 1850.

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theory of probabilities which he has recommended to the attention of students. However, in the proof which the reviewer refers to, which is contained in the tract entitled *Theoria Motus Elliptici*, Gauss undoubtedly does assume that the arithmetical mean is the most probable value in the case of direct observations of a single element. From this assumption, he shows that the probability, that the magnitude of an error lies between x and $x + dx$, must be

$$\frac{h}{\sqrt{\pi}} e^{-h^2 x^2} dx,$$

h being an indeterminate constant. It follows from this, that the results of the method of least squares are always the most probable values that can be assigned to the unknown elements. Without referring to the *Theoria Motus*, you can see the details of Gauss's reasoning in a paper by Bessel, of which a translation appeared in Taylor's Scientific Memoirs. The reviewer is right in saying that Gauss was not entitled to assume that the arithmetical mean is the most probable value. But when he speaks of this as a thing to be proved, and not assumed, we are led to suppose that he believes that subsequent writers have actually proved it. In truth this appears, not only from his statements, but also from the illustrations of which he has made use. Thus he states that if shots are fired at a wafer which is afterwards removed, and we are asked to determine from the position of the shot-marks the most probable position of the wafer, "the theory of probabilities affords a ready and precise rule, applicable not only to this but to far more intricate cases;" and he goes on to say that it may be shown that the most probable position of the wafer is the centre of gravity of the marks. Now this result is only then true when the law of probability of error, which is implied in Gauss's assumption, really obtains; so that, according to the reviewer, the demonstration of the principle of least squares must amount to showing that this law obtains universally; or, which is the same thing, that the arithmetical mean is always the most probable value in the case of direct observations of a single element. If this can be proved, it is doubtless a very curious conclusion; but it is at any rate certain that Laplace has not proved it, of whom however the reviewer asserts that he has given a rigorous demonstration of the principle of least squares. From one end of Laplace's great work to the other, there is nothing to justify the assertion that the centre of gravity of the shot-marks is the most probable position that can be assigned for the wafer; that is, that the concurrent existence of the deviations or errors which must

have taken place if the wafer really occupied this position is more probable than that of those which are similarly implied in any other hypothesis as to its place. If you find anybody sceptical as to this, pray ask them to point out the passage, either in the introductory essay, or in the work itself, or in the supplements.

What, then, did Laplace demonstrate? something so unlike this, that one is disposed to wonder how he can have been thus misunderstood. The method of least squares is simply a method for the combination of linear equations, of which the unknown quantities are the elements to be determined; the constant term of each being a direct result of observation, and therefore affected by an unknown error, while the coefficients are supposed absolutely known.

If there are more equations than requisite, that is, more than elements to be determined, what is the best way of combining them? In the first place, they must clearly be combined by some system of constant multipliers, else the resulting equations, not being linear, would generally be insoluble. This condition, however, though absolutely necessary in practice, is in no way derived from the theory of probabilities. It is a merely practical limitation. The question thus narrowed is simply to determine the system of factors to be employed for obtaining the value of any particular element. The factors must of course be such that, in the final equation, the coefficient of this element may be unity, and those of the others severally equal to zero.

These conditions being fulfilled, we get a value for the element in question which is affected by an unknown error, namely the sum of the errors of observation multiplied respectively by the corresponding factors. The mean arithmetical value of this sum may in theory at least be determined, if we know the law of probability of error for each observation; and Laplace calls that system of factors the most advantageous which makes this mean value a minimum. If, however, the law of probability of error is unknown, the mean value of the error cannot be determined. Nevertheless, if the number of observations is very large, this mean value approximates to a certain limit, the form of which is independent of the law of probability. The essence of Laplace's demonstration consists in its enabling us to determine this limit. When this is done, it may easily be shown that the most advantageous system of factors, those, namely, which make this limiting mean value of the error a minimum, will give the same value to the element to be determined as the system of final equations obtained by employing the method of least squares, provided

equal positive and negative errors are equally probable. And the same is of course true with respect to the remaining elements. Thus this system of final equations gives to each element a value affected by a smaller average error than any other linear system, if the number of observations is sufficiently large. It nowise follows that these values are the most probable; that is, that the errors which must have been committed if these are the true values, form a combination *à priori* more probable than the errors which in like manner have been committed if any other set of values are the true ones. The most advantageous set of factors for determining any element depends only on the coefficients of the equations to be discussed, and not on their constant terms, which are the direct result of observation. Thus these factors are determinable, *à priori*, before the observations are made. But it is only after the observations have been made that the most probable values of the elements can be found, and then only if we know the law of probability of error. Laplace has pointed out the difference between the two investigations.

This difference, however, the reviewer does not seem to have apprehended. He plainly supposes that Laplace proves the results of the method of least squares to be the most probable results, which can only be the case, as Gauss had in effect shown, if a special law of error obtains. He therefore undertakes to prove, that for all kinds of observations this is actually the only possible law.

But for the supposed authority of Laplace, he would probably have perceived that nothing can be more unlikely than that the errors committed in all classes of observations should follow the same law; and that at any rate this proposition, if true, could only be proved inductively, and not by an *à priori* demonstration. For it is beyond question distinctly conceivable, that different laws may exist in different classes of observation; and that which is distinctly conceivable is *à priori* possible. So that we cannot prove it to be impossible, though we may be able to show empirically that it is not true.

You will probably agree with me in thinking that a wrong notion of Laplace's reasoning lies at the root of the reviewer's new demonstration. But we now come to the demonstration itself. The assumption that the law of error is in all cases the same, is, we are told, justified by our ignorance of the causes on which errors of observation depend. The law "must necessarily be general, and apply alike to all cases, since the causes of error are supposed alike unknown in all." Two remarks are suggested by this statement: in the first place, that our ignorance of the causes of error is not so great

but that we have exceedingly good reason to believe that they operate differently in different classes of observations; and in the second, that mere ignorance is no ground for any inference whatever. *Ex nihilo nihil*. It cannot be that because we are ignorant of the matter we know something about it. Or are we to believe that the assumption is legitimate, inasmuch as it in a manner corresponds to and represents our ignorance? But then what reason have we for believing that it can lead us to conclusions which correspond to and represent outward realities? And yet the reviewer at the conclusion of his proof asserts, that, on the long run, and *exceptis excipiendis*, the results of observation "will be found to group themselves according to one invariable law." Thus the assumption, though "it is nothing more than the expression of our state of complete ignorance of the causes of error and their mode of action," leads us by a few steps of reasoning to the knowledge of a positive fact, and makes us acquainted with a general law, which is as independent of our knowledge or our ignorance as the law of gravitation.

Let us, however, suppose it to be true that the law of error is always the same, and that equal positive and negative errors are equally probable. To determine the special form of the law, the reviewer employs a particular case—he supposes a stone to be dropt with the intention that it shall fall on a given mark. Deviation from this mark is error; and the probability of an error r may be expressed by the function $f(r^2)$ or $f(x^2 + y^2)$, the origin of coordinates being placed at the mark. It is of course supposed that equal errors in all directions are equally probable. We have now only to determine the form of f . This the reviewer accomplishes in virtue of a new assumption, namely, that the observed deviation is equivalent to two deviations parallel respectively to the coordinate axes, "and is therefore a compound event of which they are the simple constituents, therefore its probability will be the product of their separate probabilities. Thus the form of our unknown function comes to be determined from this condition, viz. that the product of such functions of two independent elements is equal to the same function of their sum." Or in other words, we have to solve the functional equation

$$f(x^2)f(y^2)=f(x^2+y^2).$$

But it is not true that the probability of a compound event is the product of those of its constituents, unless the simple events into which we resolve it are independent of each other; and there is no shadow of reason for supposing that the occurrence of a deviation in one direction is independent of that

of a deviation in another, whether the two directions are at right angles or not. Some notion of an analogy with the composition of forces probably prevented the reviewer from perceiving that, unless it can be shown that a deviation y occurs with the same comparative frequency when x has one value as when it has another, we are not entitled to say that the probability of the concurrence of two deviations x and y is the product of the probabilities of each. Without this subsidiary proof, the rest of the demonstration comes to nothing. The conclusion to which it leads is in itself a *reductio ad absurdum*. Of the above written functional equation the solution is $f(x^2) = e^{mx^2}$, m being a constant, so that the probability of an error of the precise magnitude x is a finite quantity; and I need not point out to you that it follows from hence, that the probability of an error whose magnitude lies between any assigned limits is equal to infinity,—a result of which the interpretation must be left to the reviewer. He may have thought that the exponential factor is the essential part of the expression

$$\frac{h}{\sqrt{\pi}} e^{-h^2 x^2} dx,$$

and that the others might, for the sake of simplicity, be dropt out. But whatever his views may have been, his conclusion is unintelligible.

The demonstration may, however, be amended so as to avoid this difficulty, and we will suppose that the reviewer meant something different from what he has expressed. Let $f(x^2)dx$ be the probability of a deviation parallel to the axis of abscissæ, of which the magnitude lies between x and $x+dx$. Then $f(y^2)dy$ is similarly the probability of a deviation parallel to the axis of ordinates, and lying between y and $y+dy$. Thus the probability that the stone drops on the elementary area $dx dy$, of which the corner next the origin has for its coordinates x and y , seems to be $f(x^2)f(y^2)dx dy$; and as all deviations of equal magnitude are equally probable, this probability must remain unchanged as long as the sum of the squares of x and y remains the same; so that we have for determining the unknown function the equation

$$f(x^2)f(y^2) = f(0)f(x^2 + y^2),$$

of which the solution is

$$f(x^2) = A e^{mx^2};$$

and as the deviation must of necessity have some magnitude

included between positive and negative infinity, we must have

$$A \int_{-\alpha}^{+\alpha} e^{mx^2} dx = 1.$$

Hence m must be negative; if we call it $-h^2$, it is easy to show that A is equal to $\frac{h}{\sqrt{\pi}}$; so that finally

$$f(x^2) = \frac{h}{\sqrt{\pi}} e^{-h^2 x^2},$$

which is what may be called Gauss's function.

But to this demonstration, though it leads to an intelligible conclusion, the original objection still applies: the probability that the stone drops on the elementary area $dx dy$ is not, generally speaking, equal to $f(x^2)f(y^2)dx dy$; so that the equation for determining the form of the function, namely,

$$f(x^2)f(y^2) = f(0)f(x^2 + y^2),$$

is not legitimately established.

To illustrate this, let $\varpi(xy)dx dy$ be the probability that the stone falls on the elementary area in question; then the condition that the probability of a deviation of given magnitude is constant will be expressed by

$$\varpi(xy) = \varpi(\sqrt{x^2 + y^2}, 0). \quad . \quad . \quad . \quad (A.)$$

Moreover, we shall plainly have

$$f(x^2) = \int_{-\alpha}^{+\alpha} \varpi(xy) dy$$

and

$$f(y^2) = \int_{-\alpha}^{+\alpha} \varpi(xy) dx;$$

and in order that the demonstration may be valid, we must have

$$f(x^2)f(y^2) = \varpi(xy),$$

or

$$\int_{-\alpha}^{+\alpha} \varpi(xy) dy \int_{-\alpha}^{+\alpha} \varpi(xy) dx = \varpi(xy). \quad . \quad . \quad . \quad (B.)$$

If this be true, then, and then only, equation (A.) may be replaced by

$$f(x^2)f(y^2) = f(0)f(x^2 + y^2).$$

But in order that (B.) may be true, $\varpi(xy)$ must evidently be the product of two factors; one of them a function of y only, and the other of x , and the integral of each factor taken between infinite limits must be equal to unity. Combining this

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 conclusion with (A.), we find that

$$\varpi(xy) = \frac{h^2}{\pi} e^{-h^2(x^2+y^2)},$$

and consequently

$$f(x^2) = \frac{h}{\sqrt{\pi}} e^{-h^2x^2}.$$

Consequently the equation for determining the form of f results from a tacit predetermination of that function.

The assumption expressed by

$$f(x^2)f(y^2) = \varpi(xy)$$

is therefore either a simple mistake or a *petitio principii*: the former, if it is deduced from the general principle that the probability of a compound event is equal to the product of those of its elements; the latter, if it is made to depend on the particular form assigned to $f(x^2)$.

After all, too, if the demonstration were right instead of wrong, it would not prove what is wanted. For if the law of probability of a deviation parallel to a fixed axis is expressed by the function

$$\frac{h}{\sqrt{\pi}} e^{-h^2x^2} dx,$$

which is what the amended demonstration tends to show, the probability that the stone falls on the area $dx dy$ is plainly

$$\frac{h^2}{\pi} e^{-h^2(x^2+y^2)} dx dy.$$

Transforming this to polar coordinates, and integrating from 0 to 2π for the angle vector, we get $2h^2e^{-h^2r^2}rdr$ for the probability that the deviation from the mark lies between r and $r+dr$; a result which may be verified by integrating for r from zero to infinity, the integral between these limits being equal to unity. Thus if the deviations measured parallel to fixed axes follow the law which the reviewer supposes to be universally true, the deviations from the centre or origin follow quite another; and hence it appears that his illustration is altogether wrong. For if $2h^2e^{-h^2r^2}rdr$ is the probability of an error lying between r and $r+dr$, the centre of gravity of the shot-marks is not the most probable position of the wafer. So that his hypothesis is self-contradictory.

The original source of his error was probably the analogy between Gauss's law, and the limiting function in Laplace's investigation.

I am, my dear Sir,

Most truly yours,

Brighton, Sept. 19.

R. L. ELLIS.

XLI. *An Account of some Thunder-storms and extraordinary Electrical Phænomena that occurred in the neighbourhood of Manchester on Tuesday the 16th of July 1850. By PETER CLARE, F.R.A.S., Vice-President of the Literary and Philosophical Society of Manchester*.*

[With a Plate.]

ON the 16th of July in the present year several severe storms of thunder, lightning, hail and rain, attended with fatal results, occurred in the southern part of Lancashire and northern part of Cheshire, which were succeeded by some very extraordinary electrical appearances, such as I do not remember to have previously noticed, although I have been an attentive observer of electrical discharges in the atmosphere for more than half a century.

These storms, with one exception (which occurred in Derbyshire), appear to have originated in different localities, within a range of fifty miles from north to south, and forty miles from east to west; extending from the river Ribble below Preston in the north to the neighbourhood of Nantwich in the south, and from the course of the river Irwell near Bury in the east, to North Meols in the west.

For several days previous to that on which the storms occurred, the weather had been generally very fine, the barometer varying only from 30·15 inches to 30 inches, giving a mean for the three preceding days of 30·07 inches. The thermometer for the same period, at eight o'clock in the morning, varied from 65 to 67 degrees of Fahrenheit; at two o'clock P.M., from 77 to 79 degrees; and at ten o'clock in the evening, from 65 to 69 degrees; giving a mean for the four days of 70·33 degrees; whilst during the same period the wind near the earth blew from the north, or north-north-east, and the clouds during the whole time moved from east to west.

On the morning of the 16th, previous to the commencement of the storms, the weather was very fine at Manchester, with some thin clouds floating in the atmosphere; but as the day advanced the clouds became more dense, although the pressure of the air did not vary more than the one-hundredth part of an inch from eight o'clock in the morning until ten o'clock in the evening. About two o'clock in the afternoon some dark clouds had formed in the north and east, and afterwards extended towards the west; at four o'clock those in the north-west had become much more dense and dark, and one or two distant peals of thunder were heard in that direction, but with-

* Communicated by the Author; having been read to the Literary and Philosophical Society, October 1, 1850.

out the usual signs of an approaching storm. It appears, however, that about this time a violent storm of thunder and lightning had commenced in the neighbourhood of Bolton, twelve miles north-north-west of Manchester, accompanied with very heavy rain and large hail-stones, and continued for nearly two hours, extending westward, and to the west-south-west and north-west, for many miles; it also extended five or six miles to the east, as far as Bury, a distance of nine miles north of Manchester; but the rain and hail were not quite in such profusion as at Bolton, or to the north-west of it, nevertheless a boy and horse were killed by the lightning about a mile and a half beyond and to the north of Bury: the boy was about ten years of age, and riding on a horse with milk cans attached to it; and when they got to Littlewood Cross they were struck by the electric fluid, and both boy and horse killed on the spot. During the storm a house near Union Square, Bury, was also struck by the lightning, but not much damaged.

At six o'clock the rain ceased at Bolton for nearly an hour, after which the thunder, lightning and rain recommenced to the west, north-west, and east of the town, and continued for some time. The damage done within the borough of Bolton was much less than might have been expected, considering the severity of the storm; yet there were not less than seven houses and mills struck with the lightning; but the damage actually done was not to any great extent, as the fires which it occasioned were soon subdued. The effects of the storm were however more appalling in a west and north-westerly direction, to a distance of several miles, where the rain descended in torrents, causing the water to rush down the hills with immense force, covering whole meadows, carrying abundance of hay with it, and overflowing the banks of rivers to a considerable extent, by which one of the trains was stopped at the Horwich station on the Bolton and Preston Railway for about twenty minutes.

In the district below the Rivington Hills the water advanced so unexpectedly, so rapidly, and with such impetuosity, as to remove whole bales of cotton from the mills, and also pieces of cloth from the print-works, to a considerable distance. Other casualties also occurred, comprising the following, some of which are of a melancholy character.

At Horwich the flood rose to such a height, that the water burst through the windows of the cotton mill of William Bennett, jun., at Wilderswood, doing damage to the amount of several hundred pounds; other mills in the same neighbourhood also sustained considerable injury from the flood. At

Adlington, a colt belonging to Ralph Shaw was drowned by the flood. At Blackrod, Joseph France, aged forty years, was engaged with another man in sinking a shaft at a colliery, when the water rushed so suddenly into it that he could not be got out and was drowned.

At Burnden the head gearing of a chain belonging to a coal-pit of Mr. Scowcroft's was struck by the lightning and damaged; and in the same neighbourhood, a chimney-piece in the house of Thomas Braughton was struck and shattered.

At West Houghton, a large stack of hay belonging to Thomas Woods was set on fire, but soon extinguished.

At Adlington, the electric fluid entered a house where a woman and her five children were sitting, and after breaking a looking-glass that hung over her head, and destroying the chimney ornaments, it left the house without injuring any of the inmates.

At Horwich, the electric fluid entered the house of Mr. Welsh, broke a large mirror and sundry other articles, and struck a boy twelve years of age, who afterwards lay in a precarious state for some time.

At Dobhill, to the west of Bolton, a cow belonging to Peter Boardman was killed in a field by the lightning.

At Lostock, Ralph Shaw had a foal killed in a field from the same cause.

James Lathom, also of Lostock, had a three-years old colt and a horse both killed by the lightning.

At Belmont, a cow belonging to Benjamin Helme was killed by the lightning on the road near to the church.

At Hindley, a valuable cow belonging to John Battersby, of Castle Hill, was killed whilst grazing in a field with nine others.

At Horrocks Fold Farm, two girls, named Alice Makinson of Preston, and Ellen Longworth of Horwich, were sitting in the kitchen with four other persons, when the electric fluid came down the outside of the chimney, through the roof and the floor, and struck Alice Makinson dead on the spot: the fluid hit her on the shoulder, and passing down her body tore the sole from one of her clogs in its resistless progress. There were no appearances on the body of the other girl, Ellen Longworth, of having been struck by lightning, but she was taken out of the kitchen in a state of insensibility; and though she revived a little, and was restored to consciousness, she only lingered until five o'clock the next morning, when she died.

These instances of the destructive violence of the storm occurred chiefly to the north-west and west of Bolton, between

that town and Wigan, in which direction it seems to have progressed; for a little before five o'clock distant thunder was heard towards the east and south-east of Wigan, and about six another storm arose to the north of the last-named town, which spread or extended southward, so as to unite with that approaching from the east; and when they united, the rain began to descend in torrents, having more the appearance of the descent of a water-spout than of a shower of rain: the thunder and lightning were terrific; but I have not been able to ascertain whether there was any loss of life in this immediate neighbourhood, although it is reported that a person was killed at Wigan.

At St. Helens, which is six or eight miles to the south-west of Wigan, John Rigby, a coal miner, aged forty-six years, was looking out of an upstairs window of his house during the thunder-storm, about seven o'clock in the evening, when he was struck by the electric fluid and killed on the spot: there was a mark on his breast, and the shoe on his right foot was torn to pieces. Several persons in the same house were knocked down, but all of them recovered.

Robert Gore, a farm-labourer, of Marsh-side in North Meols, aged twenty years, was driving his master's cart home between six and seven o'clock the same evening, when the mare took fright at the noise of the thunder and ran away; the man and a little boy were riding in the cart at the time: when the master's son came up with them, he found the mare and cart on the ground, and Gore lying by the side of the cart quite dead: the horse and boy escaped.

Evan Rimmer, a farm-servant, aged eighteen years, was taking shelter with four others in a stable or shed adjoining William Wright's farm in Moss Lane, North Meols, when the building was struck by the electric fluid about seven o'clock the same evening during the thunder-storm, and they were all knocked down. Rimmer was killed; the others were put to bed and recovered.

In the country between St. Helens and North Meols, a distance of about twenty miles in a north-westerly direction, several persons sustained injury by the lightning; but excepting the three above mentioned, no fatal cases occurred. At Ormskirk, a woman who was sewing was struck blind by a flash of lightning, but recovered her sight in a few days.

From North Meols the storm passed northward towards the river Ribble and Lytham, but it does not appear that any damage was done there by the lightning.

At about eight o'clock the same evening the town of Warrington, which is situated several miles to the south of the

direction in which the storm above described had raged, was also visited by a violent storm of thunder and lightning; the lightning was intensely vivid, and the peals of thunder followed each other in rapid succession: several accidents occurred, but no fatal cases are recorded. At Sankey, a little to the north-west of Warrington, a stack of hay was set on fire by the lightning, and a large quantity burnt before the flames were extinguished. A flat sailing on the canal was also set on fire by the same cause, but the damage done was not extensive. This storm appears to have been limited to a comparatively small district.

About six o'clock the same evening a storm of thunder and lightning accompanied with heavy rain commenced about ten miles to the west of Warrington, and proceeded in a southeasterly direction. After crossing the river Mersey near Runcorn and Weston Point, it passed up the valley of the river Weaver towards Northwich, a town situated ten miles south of Warrington. At Anderton, about a mile short of the town, the lightning struck a stack of hay and set it on fire, but the torrents of rain soon extinguished it. At Witton to the east, and Leftwich to the south, and both immediately adjoining Northwich, it appears as if a discharge of electricity had struck three places at one and the same time; viz. a cottage near the toll-bar in Witton was struck, but not much damaged; a church in Leftwich was also struck, and a quantity of stone broken and forced off the gable-end by the lightning, which passed from thence to a gutter and down a metal spout to the ground; the third was a poplar-tree in Leftwich, which was shivered at the top and down the trunk to the ground. In about an hour afterwards the spire of Davenham Church, situated two miles to the south, was also struck by the lightning; it first came in contact with a wind-vane at the top of the spire, after which it passed down a copper rod about two inches diameter inserted through the solid stone-work for several yards, with a screw and nut at the bottom to hold the masonry together; and when it got to the lower end of the metal, left it and made a large hole in the spire about two yards long and one foot wide; it then proceeded along the masonry to the bottom of the spire, where it made another large opening in the stone-work, from whence it immediately entered the tower; here an iron pipe or smoke-flue on the outside of the tower served as a conductor for the lightning, which was conveyed without injury to the building as far as the metal went; but where it terminated, towards the bottom of the tower, some damage was done to the stone-work before the lightning entered the ground.

Later in the evening a cow belonging to the Rev. Mr. France of Davenham was killed in Bostock Park, about two miles further south; and soon afterwards a barn at Winsford, about a mile and a half to the west of Bostock, was struck by the lightning but not much damaged; and a little later in the evening a cow was killed at Minshull, a few miles south of Winsford.

The storm extended some distance further to the south and west, visiting in its progress Holmes Chapel, Over, Nantwich, &c.

The electrical state of the atmosphere during the progress of these storms must have been very much disturbed, as manifested by the frequency and intensity of the electrical discharges; whilst the torrents of rain that fell for some time were probably caused by the currents of air in the higher part of the atmosphere being much agitated, and moving in various directions, thereby allowing them to mix freely, and large clouds to be rapidly generated.

With the limited knowledge we have of the operation of those causes which produce thunder-storms, the following view may not be undeserving a little consideration.

If we suppose that the quantity or intensity of the electric fluid connected or combined with each particle or atom of water is not the same when the atom is in a liquid as it is when in an aëriform state, but, like heat, abounds more when the atoms are in a state of vapour than when they are in a liquid state, and which view some experiments appear to support; then, whenever a quantity of vapour is suddenly condensed in the atmosphere, the water, whether in the state of a liquid mass or in innumerable drops, would probably give out electricity, or under favourable circumstances become positively electrified; by this hypothesis we may account for most, if not all, the phænomena that occur in thunder-storms.

For if the currents of warm and cold air in the atmosphere are in a very disturbed state, moving in opposite or various directions, and both nearly saturated with vapour, and if under such circumstances they become mixed, a portion of the vapour in the warm air will be condensed and form clouds: the clouds would be electrified with an intensity proportioned to their density, magnitude, rapidity with which they were formed, and the hygrometric state of the air between them and the earth; and if sufficiently electrified, would remain in masses separated from each other: this appearance is often observed in the vicinity of an electrified cloud, or previous to a thunder-storm. And further, as clouds are generally of different magnitudes and densities, the electrical power of

those most intensely charged will cause the electricity of the sides, or portions of other clouds nearest to them, to be electrified in a different state, according to the well-known laws of electrical induction: and as the larger clouds increase in density or electrical intensity, they will discharge portions of their electricity to the smaller and less intensely charged clouds; these again may discharge themselves to more remote clouds that are still more slightly charged, and thus a succession of flashes of lightning and peals of thunder may occur for some time. But if the general state of the atmosphere below the clouds be very damp, the superabundant electricity would be quietly conveyed to the earth without producing any electrical appearances whatever.

In those regions of the atmosphere where clouds float or are formed at a considerable elevation, and where the air is much more rare than near the surface of the earth, the resistance to the passage of electricity through it is much less than near the ground, and consequently the discharges from cloud to cloud will be more frequent and to greater distances than from the clouds to the earth.

If the clouds are rapidly formed and discharge their electricity frequently to the earth, it is probable that a very large amount of rain will ensue; for during the time they are charged, the small particles of water of which they are composed will be repelled from each other by the diverging power of their electricity; whilst the moment a discharge takes place, especially if it be to the earth, the electricity which kept the particles from uniting together being removed, the drops of water will immediately unite in immense quantities, and, falling to the earth, will suddenly increase the shower, not only in quantity but in the size of the drops, as is frequently noticed by attentive observers. As the different strata of air continue to mix, the clouds increase and again become charged with electricity, the drops again diverge by repulsion, and the rain ceases to fall as copiously as it did soon after the discharge of electricity to the earth; in this manner we may account for the occasional change during a thunder-storm, from an immense profusion to a moderate fall of rain, and *vice versâ*.

In a thunder-storm prevailing over a considerable extent of country, and with the clouds at a great elevation, the discharges of electricity may pass several miles through the air from one cloud to another; in such cases an observer may have considerable difficulty to ascertain in what portion of the sky the lightning has prevailed; but our late eminent president, Dr. Dalton, has elegantly described, at page 203 of the second edition of his Meteorology, how the difficulty can be explained.

The electrical state of the atmosphere must have been greatly disturbed for a much wider district than has been described ; but though we have no account of a thunder-storm having occurred on the 16th of July between Bury and the Derbyshire Hills, yet beyond them there was a severe storm ; for Mr. Ransome, F.R.C.S., informs me he was travelling on that day between Matlock and Buxton, and whilst on the railway, before arriving at Rowsley, they experienced a violent storm of thunder, lightning and rain, about five o'clock ; but on arriving at Buxton he did not hear that the storm had visited that neighbourhood.

In the country between Buxton and Holmes Chapel in Cheshire, a distance of twenty-five miles from east to west, and with some lofty hills to the west of Buxton, there was not any severe storm of thunder and lightning on that day ; but some sheet lightning and distant thunder were noticed over a considerable extent of that country in the course of the evening, with a little rain about sixteen or eighteen miles to the west of Buxton.

The storms herein described have no extraordinary features, except their violence and the melancholy casualties that accompanied them ; but they exhibit a case of electrical disturbance deserving notice in connexion with, or immediately preceding the very extraordinary appearances that occurred in the course of the same evening.

As the evening advanced, the sheet lightning became more frequent and vivid at Manchester ; and before nine o'clock the clouds in the south-south-west and west had become very dark, whilst those towards the south and south-east were not near so dense, and were separated into masses, with open spaces between them ; these spaces became very plainly visible when the sheet lightning occurred, which about this period was very frequent, and accompanied with distant thunder.

From a quarter before until half-past nine some very extraordinary appearances of lightning were observed, such as I never before witnessed ; several flashes seeming to be almost continuous, or repeated at such short intervals as were scarcely appreciable, whilst at other times the light actually continued for a considerable portion of a second.

The bright coruscations of the electric fluid, which on ordinary occasions pass between one cloud and another, or between a cloud and the earth, in a tortuous or zigzag line, on this occasion presented a great variety of forms and ramifications towards the south and south-south-west, similar to the accompanying sketches, and at an elevation of from fourteen to twenty degrees above the horizon (see Plate I.) ; sometimes

they appeared branched like the roots of a tree, and occasionally with bright balls at the termination of all or some of the branches. These coruscations of light commenced about the south-west by south, and in all cases proceeded from west to east, or from right to left, passing through a horizontal space of from eight to fourteen degrees; and at times the motion of the electric fluid appeared to be so slow, that its progress could be easily observed.

On three or four occasions, immediately after a ray or narrow line of light had passed through a horizontal space of ten or twelve degrees, a luminous ball of considerable size, more than twice the diameter of Venus when at her greatest brilliancy; suddenly appeared, and moved along in the same direction as the ray of light had passed, with a progressive motion from right to left, as from A to B in figs. 2 and 6, and occupied at least the tenth of a second in its progress. The other appearances were of a similar character to what are given in the different figures, and their motion varied from a horizontal to a vertical direction, as much as the position of the figures represented in the sketches.

Some of the coruscations of the electric fluid terminated with a bright ball at the extremity of each branch, as in fig. 5; whilst at other times bright balls were seen at only two or three branches, as at fig. 7. In all cases these luminous appearances seemed to commence from a point where the clouds were not dense or dark, and to proceed through the air to their termination without entering or being obscured by a cloud.

These, or similar phænomena, were observed by Mr. Ransome and his son at Fairfield, near Buxton, in a westerly direction, about eight o'clock; but with this difference, that the branches appeared to pass from left to right, whilst those I witnessed passed from right to left; similar appearances were seen by Mr. Chrimes to the north of the zenith, about a mile beyond Wilmslow in Cheshire, who also observed that the branches passed from left to right, beginning in the west and moving towards the north-east; and in some cases they were so near the ground, that when they disappeared he thought they were in contact with the hedges.

Edward Brooke, Esq., of Marsden House, about five miles to the east of Stockport, with some friends who were on a visit to him, also observed these remarkable coruscations, which he says proceeded from left to right, similar to those observed near Buxton and Wilmslow; and Colonel Stott, who was of the party, and who had been several years resident in the East Indies, declared he had very frequently noticed the dis-

play of lightning in that part of the world, but never saw anything like the coruscations that appeared that evening.

Mrs. Clayton, of Adlington in Cheshire, whose residence is situated three or four miles to the south of Marsden House, also observed these brilliant coruscations, and likewise says they appeared to proceed from left to right, or in a direction contrary to the drawings in the Plate.

Mr. Alderman Shuttleworth informs me that his sister-in-law observed these remarkable coruscations of lightning in the neighbourhood of Bolton, which, being twelve miles to the north-north-west of Manchester, gives a much wider extent of country in which they were observed than the previous accounts have stated. In the extract of a letter from her which he has sent, she says, "I was at Bolton on the 16th of July, and witnessed an awful storm of thunder and lightning * * * * about eight a servant who was watching at an upper window came to say that she saw rings of fire in the sky. There was a large black cloud in the south-west; behind it the heavens appeared to open, throwing forth sometimes showers of brilliant sparks or of balls of fire, sometimes circles of flame, sometimes fiery serpents, and at others forked lightning of unusual breadth, the clouds always edged with beautiful sheet lightning."

Mr. Joule, F.R.S., has published a highly interesting account in the *Philosophical Magazine* for August, of these remarkable phænomena as observed by him. He says, some of the coruscations passed across the zenith; and from the time that elapsed between seeing them and hearing the thunder, he considers their general elevation to have been about three miles and a half: he likewise observed that the branches moved from right to left, similar to what I saw, as shown in the accompanying figures. His residence is about a mile and a quarter to the west-north-west of mine. He has given a sketch in the *Magazine* of the appearance observed by him, which terminated in more numerous branches than those I noticed; but though I did not observe any branches so much fimbriated at the end as he has represented, yet Mrs. Clayton, to whom I showed his account, said she saw some branches very much like the figure accompanying his paper, but with curves at the ends bent more inwards than in his figure.

With regard to the identity of these luminous appearances seen by different individuals, and the apparent difference in the direction of their motion, as stated by the observers at Buxton, Marsden House, Adlington and Wilmslow, compared with the account given by the observers at Manchester and its vicinity, it may be remarked that Marsden House is

to the south of east, Buxton and Adlington to the south-east, and Wilmslow nearly south of Manchester. Now if we suppose these coruscations to have moved from south-west to north-east, and some of them at no great elevation above the earth or distance from Manchester, they would appear to move from right to left at that town or its vicinity; but if the same were observed at Marsden House, Adlington or Wilmslow, they would appear to move in an opposite direction with regard to the spectator, or from left to right, although in both cases their motion would actually commence in the south-west and be continued towards the north-east.

Considering the distance of Buxton from Manchester, it is not likely that the same coruscations would be seen at both places; nor would those observed near Buxton be identical with those seen near Marsden House, Adlington or Wilmslow, on account of the lofty hills intervening, and the low elevation of the electrical discharges seen at the latter place; it is therefore probable that these discharges of the electric fluid were not confined to a very limited space, but prevailed in the atmosphere over a considerable district of country, and at a very moderate elevation; but there is not sufficient evidence to enable us to determine either their height, or to what extent they prevailed.

Mr. Chrimes states that he did not hear any sound as if it proceeded from the coruscations of light which he observed in the neighbourhood of Wilmslow; although at the same time he heard distant thunder in the west, but not any sound in the north or north-east, although that was the direction in which the lights were observed to pass and disappear. And as all parties agree that these brilliant ramifications did not proceed with the usual velocity of lightning, is it not probable that their motion was not sufficiently rapid to cause such a violent concussion in the air as to produce sound?

There were no very dense clouds where the coruscations appeared; but in the same direction the sky was mostly obscured with clouds of different heights, some of which, as well as various strata of the air, were probably in different states of electrification, whereby the electric fluid might be induced to pass from clouds positively electrified to those in a negative state, or to a stratum of air negatively electrified. In the passage of electricity from a body positively electrified, it frequently becomes divided into various branches, especially as it approaches the negative body: this is often illustrated in the progress of electrical discharges from the clouds to the earth, when they are observed to be divided into several branches as they approach the ground. Similar appearances

are often noticed in strong electrical discharges with a powerful machine, especially where the electric fluid has to be diffused on or amongst imperfect conductors.

Probably these or similar phænomena are not uncommon in the torrid zone, where it is said the coruscations of lightning are frequently seen in the sky when there are no clouds; but as similar appearances are rarely, if ever, observed in this neighbourhood, I have been induced to draw up the foregoing account in the hope that if similar phænomena have been noticed, some description of them may be given by other writers.

Manchester, September 30, 1850.

XLII. *Essay on the Theory of Attraction.* By JOHN KINNERSLEY SMYTHIES, *Barrister-at-Law of the Middle Temple.*

To the Editors of the Philosophical Magazine and Journal.

3 Oakley Square, London,
October 14, 1850.

GENTLEMEN,

I ADMIT that my essay noticed in your last Number contains a serious error. I now perceive that wherever ϕ (the function of the distance, according to which the attractive force varies) occurs in an equation, the term involving it is multiplied by a function of the distances and angles, which is zero for all positions of the bodies, so that ϕ may have any value consistently with the equations containing it. The correction of this error requires that sections 11, and 13–18 inclusive, and some short references to them, should be cancelled: the remainder will be free from this error.

Till I read your notice, I supposed that my equation between the ten mutual distances of five points in space was new; and having since referred to Carnot's memoir, I find that the labour I spent on the solution of that problem is not wholly lost, since my demonstration is much shorter and less laborious for the reader than Carnot's. When all, in which I have been anticipated or have erred, shall have been deducted from my essay, I venture to express my hope that something new and true will still remain. By the publication of this short statement in your Magazine you will much oblige, Gentlemen,

Your obedient Servant,

J. K. SMYTHIES.

XLIII. *On the Diffusion of Liquids.*
By THOMAS GRAHAM, F.R.S., F.C.S.

[Concluded from p. 281.]

III. DIFFUSION OF SALTS OF SODA.

(1.) **T**HE only salts of soda which I have yet had an opportunity of diffusing in a sufficient variety of circumstances are the carbonate and sulphate. These salts appear to be equidiffusive, but to diverge notwithstanding more widely in the solutions of the higher proportions of salt than the corresponding potash salts. It is a question whether this increased divergence is not due to the less solubility of the soda salts, and the nearer approach consequently to their points of saturation in the stronger solutions.

Table XIII.—Diffusion of Carbonate and Sulphate of Soda.

Parts of anhydrous salt to 100 water.	Density of solution at 60°.	At 64°.		At 37°7	
		Experi- ments.	Mean.	Experi- ments.	Mean.
Carbonate of soda					
2	1·0202	4·15 4·08 4·21		2·78 2·62 2·73	
4	1·0405	7·96 7·70 7·68	4·14 7·78	5·31 4·94 5·35	2·71 5·20
6 $\frac{2}{3}$	1·0653	12·16 12·06 12·45		8·50 8·45 8·05	
10	1·0957	17·13 16·53 17·00	12·22 16·88		8·33
Sulphate of Soda					
2	1·0179	4·35 4·32 4·25		2·96 3·03 3·09	
4	1·0352	8·14 8·10 8·28	4·31 8·17	5·63 5·64 5·42	3·03 5·56
6 $\frac{2}{3}$	1·0578	13·26 13·63 13·61		8·77 8·84	
10	1·0847	18·71 19·73 18·91	13·50 19·14		8·80

The range of the thermometer during the continuance of the experiments at the higher temperature was from 64°·5 up to 65° and falling again to 63°; the mean of all the days being 64°. The temperature of the other series, or of the ice-box,

was 42° the first day, 38° the second, and 37° steadily for the remainder of the period; the mean being 37°·7.

The mean results at 64° are as follows:—

	2.	4.	6 $\frac{2}{3}$	10.
Carbonate of soda	4·14	7·78	12·22	16·88
Sulphate of soda	4·31	8·17	13·50	19·14

Another series of experiments was made upon a 1 per cent. solution of the same salts at a mean temperature of 64°·9. Six phials of each solution were diffused, and the water of two jars afterwards evaporated together, so that the quantities stated are double.

The diffusion product in three experiments with the sulphate of soda was 4·77, 4·75 and 4·80 grs.; mean 4·77 grs. The diffusion product in three experiments with the carbonate of soda was 4·61, 4·68 and 4·67 grs.; mean 4·65 grs. The difference between the carbonate and sulphate is 0·12 gr.; it is less for the present proportion of 1 per cent. of salt, than for 2 per cent., so that the diffusion of the salts may be converging to a perfect equality in very weak solutions. One-half of the preceding quantities, or the mean results for a single diffusion cell, are—

Diffusion of 1 per cent. solutions at 64°·9.

Carbonate of soda, 2·32 grs. . . 100

Sulphate of soda, 2·38 grs. . . 102·58

(2.) The diffusion of the carbonate of soda was further compared with the nitrate of the same base, to find whether their times of equal diffusion are related like those of the corresponding potash salts. The mean temperature of the first seven days, which was the period of diffusion for the nitrate of soda, was 66°·9; of the last three days, 65°·2; and of the whole period of 9·9 days occupied by the carbonate of soda, 66°·4. The 4 per cent. solutions were employed.

The nitrate of soda gave a diffusion product, in three experiments, of 11·48, 11·58 and 12·13 grs.; mean 11·73 grs.

The carbonate of soda, in three experiments, gave 11·66, 11·53 and 11·52 grs.; mean 11·57 grs. A slight addition should be made to the latter quantity to raise the diffusion product from 66°·4 to 66°·9. It will appear from a subsequent experiment that the diffusion of the carbonate of soda increases 0·096 gr. for a rise of one degree of temperature; which will give 0·05 gr. for the half degree in question.

Bringing the diffusion of the two salts to the same temperature of $66^{\circ}9$, we have therefore diffused, of—

Nitrate of soda, in seven days, $11^{\circ}73$ grs. .	100
Carbonate of soda, in $9^{\circ}9$ days, $11^{\circ}62$ grs. .	99.06

The difference in the quantity diffused of the two salts is only 0.11 gr., or 1 per cent., which is quite within the unavoidable errors of observation.

(3.) The diffusion of a 2 per cent. solution of the same salts was repeated at the same inferior temperature of $54^{\circ}3$ as with the salts of potash, and under the same difficulties from fluctuation of atmospheric temperature. Two water-jars were evaporated together, so that the results are double.

Nitrate of soda, diffused for seven days at a mean temperature of $54^{\circ}3$, gave 10.15, 10.24 and 9.92 grs. in three experiments; mean 10.10 grs.

Carbonate of soda, diffused for $9^{\circ}9$ days at a mean temperature of $53^{\circ}4$, gave 9.93, 9.54 and 10.10 grs. in three experiments; mean 9.86 grs. But the latter amount is to be increased by 0.09 gr. to bring it to the diffusion of $54^{\circ}3$. We have then for the diffusion product of the two salts at the same temperature of $54^{\circ}3$ —

Nitrate of soda, in 7 days, 10.10 grs. .	100
Carbonate of soda, in $9^{\circ}9$ days, 9.95 grs. .	98.51

The difference is again small, namely, 0.15 gr., or $1\frac{1}{2}$ per cent., and within the limits of unavoidable error.

It appears therefore that the times of equal diffusion of the nitrate and carbonate of soda are related like those of the nitrate and carbonate of potash, or as the square root of 1 and 2, that is, as 1 to 1.4142.

Relation of Salts of Potash to Salts of Soda.

It appeared probable, from many of the experiments already recorded, that if any relation, in the times of equal diffusibility, existed between the corresponding salts of potash and soda, it was that of the square root of 2 to the square root of 3. They were accordingly diffused for times having this ratio; namely, the nitrate of potash for seven days, the nitrate of soda for 8.57325 days; the sulphate and carbonate of potash for 9.9 days, and the sulphate and carbonate of soda for 12.125 days. If these times are rightly chosen, the eventual diffusion products of all the experiments should be equal. The 1 per cent. solution was selected, and the number of experiments simultaneously made on each salt was eight or six. The liquids of two water-jars were evaporated together, so that each of the

results in the table below represents the diffusion of two cells. These experiments also afford another opportunity of testing the assumed relation between the nitrates and sulphates of the same base.

Table XIV.—Solution: 1 Salt to 100 Water, at $55^{\circ}\cdot4$ — $56^{\circ}\cdot1$.

	Temperature.	Time in days.	Square of times. Sol. density.	Diffusion product of two cells in grs.				
				Exp. I.	Exp. II.	Exp. III.	Exp. IV.	Mean.
Nitrate of potash	$55^{\circ}\cdot1$	7	2	6·67	6·87	6·90	6·57	6·75
Nitrate of soda ...	$55^{\circ}\cdot7$	8·57	3	6·59	6·80	6·94	6·57	6·78
Sulphate of potash	$55^{\circ}\cdot4$	9·90	4	6·73	6·77	6·96	6·68	6·78
Sulphate of soda...	$55^{\circ}\cdot4$	12·125	6	6·43	6·94	6·80	6·68	6·72
Carbonate of potash	$55^{\circ}\cdot4$	9·90	4	6·54	6·64	6·40	6·67	6·56
Carbonate of soda	$55^{\circ}\cdot4$	12·125	6	6·40	6·63	6·60	6·67	6·54

The range of temperature during the period of these experiments rather exceeded 3 degrees, so that they cannot be considered as fortunate in that respect; but still the similarity between the different sets of experiments, and the near equality of their means, is very remarkable. The two nitrates and the two sulphates may be said to coincide, the extreme difference of the means of the four salts not being quite so much as 1 per cent. The two carbonates fall about $3\cdot4$ per cent. below the sulphates and nitrates, but agree perfectly with each other, showing a uniformity in their irregularity. This deviation of the carbonates would appear essential, as it has been observed every time they have been compared with the sulphates.

The double relation between salts of potash and salts of soda, and between the nitrate and sulphate class of each of these bases, will, I believe, be allowed to acquire considerable additional support from this new series of observations.

IV. DIFFUSION OF SULPHATE OF MAGNESIA.

In a set of preliminary experiments upon sulphate of magnesia in comparison with sulphate of potash, the 4 per cent. solutions of both salts were diffused for seven days at a mean temperature of $57^{\circ}\cdot9$, with very little fluctuation, the extreme range being from $58^{\circ}\cdot5$ to $57^{\circ}\cdot75$. The sulphate of magnesia is taken anhydrous in all the following experiments. The diffusion of sulphate of potash in three cells was 9·16, 9·22 and 9·57 grs.; mean 9·32 grs.

The diffusion of sulphate of magnesia in three cells was 5·21, 4·98 and 5·34 grs.; mean 5·18 grs. The diffusion, in

equal times, appears here to be as 100 sulphate of potash to 55.58 sulphate of magnesia. We know, however, when unequally diffusible salts are diffused for equal times, that the diffusion of the slower is exaggerated. Consequently the diffusion of sulphate of magnesia is likely to be represented in excess in these experiments.

In a second preliminary series of experiments the same 4 per cent. solutions were diffused, the sulphate of potash for eight days and the sulphate of magnesia for nineteen days, with the view of discovering their times of equal diffusibility.

During the first period of eight days the temperature fluctuated considerably, beginning at 54° , falling gradually in four days to $50^{\circ}.5$, and rising again in four days to 53° ; the average of the whole period was $52^{\circ}.2$. The diffusion of sulphate of potash from three cells was 9.36, 9.25 and 10.52 grs.; mean 9.71 grs.

During the second period of nineteen days, which included the first period, the mean temperature was $54^{\circ}.6$. The diffusion of sulphate of magnesia from three cells was 11.81, 11.61 and 10.90 grs.; mean 11.44 grs. The variation in the amounts diffused of both salts is greater than usual, owing no doubt to the changes of temperature, which were imperfectly controlled.

Dividing the quantity of salt diffused by the number of days, we have of sulphate of potash 1.214 gr. diffused per day, and of sulphate of magnesia 0.602 gr. per day; or the latter salt exhibits sensibly half the diffusibility of the former in equal times. This suggested the trial of times for these two salts in the proportion of 1 to 2, with the view of obtaining equal diffusions.

(1.) A one per cent. solution of sulphate of magnesia (anhydrous) was diffused for the long period of 19.8 days, at a mean temperature of $54^{\circ}.7$, in eight cells. The diffusion products of four pairs of cells were 7.07, 6.71, 7.07 and 7.35 grs.; mean 7.05 grs., or for one cell, 3.53 grs.

A similar solution of sulphate of potash diffused for 9.9 days, or half the preceding period, at a mean temperature of $55^{\circ}.4$, or $0^{\circ}.7$ higher, gave a mean product, for two cells, of 6.79 grs., as before stated, or for one cell, of 3.40 grs. The diffusion of sulphate of potash being 100, that of sulphate of magnesia is therefore 103.7, a fair approximation to equality.

(2.) In a second series of experiments upon 1 per cent. solutions of the same two salts, diffused in the vault for fourteen and seven days respectively, with a mean temperature of $53^{\circ}.8$ for the sulphate of magnesia, and $54^{\circ}.3$ for the sulphate of potash, the temperature was remarkably uniform, gradually

falling from $55^{\circ}2$ to 53° during the longer period, but without any injurious oscillation.

From eight cells, evaporated two together, the sulphate of magnesia obtained was 6.12, 6.12, 6.04 and 6.03 grs.; mean 6.08 grs., or 3.04 grs. for one cell.

The sulphate of potash gave from eight cells, in experiments already detailed, a mean result of 5.84 grs. of salt for two cells, or 2.92 grs. for one cell. The diffusion is in the proportion of 100 sulphate of potash to 104.11 sulphate of magnesia, the times being as 1 to 2 for the two salts respectively.

From these two series of experiments, it appears that, at 54° , sulphate of magnesia has nearly, if not exactly, half the diffusibility of sulphate of potash, and consequently one-fourth of that of hydrate of potash. Or, the times of equal diffusion for these three salts appear to be 1, 2 and 4. The squares of these times and the solution densities are 1, 4 and 16. Hydrate of potash may possibly therefore have the same relation to sulphate of magnesia in solution, density and diffusibility, that hydrogen gas has to oxygen gas.

(3.) A two per cent. solution of sulphate of magnesia, diffused for fourteen days, gave at $53^{\circ}9$, for two pairs of cells, 9.57 and 10.00 grs. of salt, of which the mean is 9.79 grs., or 4.85 grs. for one cell.

A similar solution of sulphate of potash diffused for seven days gave a mean result of 4.97 grs. of salt for one cell, at $54^{\circ}2$, as already stated. The result is a diffusion of 100 sulphate of potash to 97.59 sulphate of magnesia.

(4.) A four per cent. solution of sulphate of magnesia, diffused for fourteen days, gave at $53^{\circ}7$, in two pairs of cells, 18.00 and 18.20 grs. of salt; mean 18.10 grs. for two cells, or 9.05 grs. for a single cell.

A similar solution of sulphate of potash, diffused for seven days at $54^{\circ}2$, gave a mean result of 9.30 grs. of salt for a single cell, as already stated. This is a diffusion of 100 sulphate of potash to 97.4 sulphate of magnesia.

The diffusion of the 2 and 4 per cent. solutions of sulphate of magnesia is so nearly equal to the diffusion of the same proportions of sulphate of potash in half the time, that they may be considered as supplying additional support to the assumed relation between the diffusibilities of these salts.

I may add, that a 4 per cent. solution of anhydrous sulphate of zinc was diffused for fourteen days, simultaneously with the similar solution of sulphate of magnesia, and of course at the same temperature of $53^{\circ}7$. Two cells, evaporated two together, gave 17.40 and 17.36 grs. of ignited sulphate of zinc; mean 17.38 grs. The salt remained, after ignition, entirely

soluble. This is a diffusion of 8·69 grs. for one cell, while the sulphate of magnesia gave 9·05 grs.; or of 100 sulphate of zinc to 104·14 sulphate of magnesia. This result is interesting, as we here find two salts which are isomorphous, and of which the equi-diffusion is on that account in a high degree probable, differing between themselves so much as 4 per cent.

Another numerous series of experiments was made at a considerably lower temperature, with the view of testing several of the same relations. The temperature in commencing the diffusion was 41°, but fell in the course of three days to 38°·8, and afterwards rose to 39°, from which it never varied afterwards more than a degree during the diffusion of the salts of potash and soda. The mean temperature for their periods did not vary above 0°·1 or 0°·2 from 39°·7, so that it may be supposed the same for all these salts. For the sulphates of magnesia, the mean temperature was 38°·9, or 0°·8 lower. The times chosen are as the square-roots of 2, 3, 6 and 16.

Table XV.—Solutions of 1 and 2 Salt to 100 Water, at 39°·7.

	Time in days.	Square of times. Sol. density.	Diffusion product of two cells in 1 per cent. solutions, and one cell in 2 per cent. solutions.				
			Exp. I.	Exp. II.	Exp. III.	Exp. IV.	Mean.
Chloride of potassium, 2 per cent....	9	2	6·58	6·79	6·82		6·73
Nitrate of soda, 2 per cent.....	11·022	3	6·66	6·98	6·79		6·81
Chloride of sodium, 1 per cent.....	11·022	3	6·33	6·63	6·73	7·06	6·69
Chloride of sodium, 2 per cent.....	11·022	3	6·50	6·60	6·64	6·74	6·62
Sulphate of soda, 1 per cent.....	15·589	6	6·60	6·56	6·56	6·50	6·55
Sulphate of soda, 2 per cent.....	15·589	6	6·50	5·43	6·33		6·42
Sulphate of magnesia, 1 per cent....	25·456	16	6·36	6·20	6·86	6·59	6·50
Sulphate of magnesia, 2 per cent....	25·456	16	6·42	6·78	6·50	6·84	6·63

Several other salts were diffused in the same circumstances as the preceding, of which the diffusion products have been previously given. Of these salts, both the 1 and 2 per cent. solutions of nitrate of potash gave 6·83 in nine days, or in the same time as chloride of potassium in the table. The latter salt maintains a sensible equality of diffusion with the present series at the low, as well as it was found to do at the former high temperature. Chloride of sodium is here introduced for the first time: it appears to be equi-diffusive with nitrate of soda. If the sulphate of magnesia diffused be increased by 0·07, for its lower temperature, this salt will be in close accordance with the salts of potash and soda.

Taking nitrate of potash 6·83, as 100, for a standard, the

salt which deviates most considerably is sulphate of soda, which for the 1 per cent. solution is 6.55, or 95.9. A low temperature, however, must be unfavourable to diffusion experiments, from increasing the tendency of salts to crystallize.

In conclusion, I may sum up the results of most interest which this inquiry respecting liquid diffusion has hitherto furnished.

1. I would place first the method of observing liquid diffusion. This method, although simple, appears to admit of sufficient exactness. It enables us to make a new class of observations which can be expressed in numbers, and of which a vast variety of substances may be the object, in fact everything soluble. Diffusion is also a property of a fundamental character, upon which other properties depend, like the volatility of substances; while the number of substances which are soluble and therefore diffusible, appears to be much greater than the number of volatile bodies.

2. The novel scale of Solution Densities, which are suggested by the different diffusibilities of salts, and to which alone, guided by the analogy of gaseous diffusion, we can refer these diffusibilities. Liquid diffusion thus supplies the densities of a new kind of molecules, but nothing more respecting them.

The fact that the relations in diffusion of different substances refer to equal weights of those substances, and not to their atomic weights or equivalents, is one which reaches to the very basis of molecular chemistry. The relation most frequently possessed is that of equality, the relation of all others most easily observed. In liquid diffusion we appear to deal no longer with chemical equivalents or the Daltonian atoms, but with masses even more simply related to each other in weight. Founding still upon the chemical atoms, we may suppose that they can group together in such numbers as to form new and larger molecules of equal weight for different substances, or if not of equal weight, of weights which appear to have a simple relation to each other. It is this new class of molecules which appear to play a part in solubility and liquid diffusion, and not the atoms of chemical combination.

3. The formation of classes of equi-diffusive substances. These classes are evidently often more comprehensive than the isomorphous groups, although I have reason to imagine that they sometimes divide such groups; that while the diffusion of salts of baryta and strontia, for instance, is similar, the diffusion of salts of lead may be different.

4. The separation of the whole salts (apparently) of potash

and of soda into two divisions, the sulphate and nitrate groups, which must have a chemical significancy. The same division of the salts in question has been made by M. Gerhardt, on the ground that the nitrate class is monobasic and the sulphate class bibasic.

5. The application of liquid diffusion to the separation of mixed salts, in natural and in artificial operations.

6. The application of liquid diffusion to produce chemical decompositions.

7. The assistance which a knowledge of liquid diffusion will afford in the investigation of endosmose. When the diffusibility of the salts in a liquid is known, the compound effect presented in an endosmotic experiment may be analysed, and the true share of the membrane in the result be ascertained.

But on the mere threshold of so wide a subject as liquid diffusion, I must postpone speculation to the determination of new facts and the enlargement of my data, of the present incompleteness of which I am fully sensible.

XLIV. *On the Crystalline Form of Beudantite.*

By H. J. BROOKE, Esq., F.R.S.*

IN a paper by Dr. Percy in the September Number of this Journal, on the chemical constitution of Beudantite, it is stated that its form is certainly very similar to, if not identical with, that of cube-ore; that Levy maintained it to be an obtuse rhombohedron with the vertical angle truncated; and that Descloizeaux, on the other hand, asserts that the crystals are cubes similar in all respects to those of cube-ore from Cornwall. It is clear from this statement that the crystals examined by Descloizeaux were different in form from those examined and described by Levy, which have *only one* of the solid angles of the supposed cube truncated, the truncating face being large in comparison with the size of the crystals; and instead of being bright, like the other faces, is of such a velvety dullness as scarcely to reflect any light.

The composition seems therefore to have influenced the form and character of the crystals of Levy's specimen, the further distinction of which from those of cube-ore cannot, however, on account of the imperfection of the faces, be made out.

* Communicated by the Author.

XLV. *Theory of Ætherification.* By ALEXANDER WILLIAMSON, *Professor of Practical Chemistry in the London University*.*.

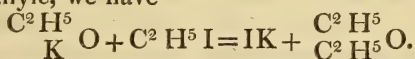
WHEN sulphuric acid is brought in contact with alcohol under certain circumstances, a new arrangement is effected in the elements of the alcohol, which divide into two groups, forming æther and water. Now it is well known that the process by which this change is effected may be represented in two ways, the difference of which consists in their respectively selecting for starting-point a different view of the constitution of alcohol. According to the one view, an atom of alcohol weighs 23, and is made up of $C^2 H^6 O$; so that to form æther, two atoms of it are needed, one of which takes $C^2 H^4$ from the other, setting free the water with which these elements were combined; whereas, according to the other view, alcohol weighs 46, and contains æther and water. These are not the only points of difference which are urged; but they are the most real and tangible, and their consideration is sufficient for our present purpose. If by any direct fact we could decide which of these two expressions is the correct one, the ground would be clear for an examination of the process of ætherification itself. In order to show more clearly the true meaning of the facts I have to adduce on this point, I will bring them before you in the order in which they arose.

My object in commencing the experiments was to obtain new alcohols by substituting carburetted hydrogen for hydrogen in a known alcohol. With this view I had recourse to an expedient, which may render valuable services on similar occasions. It consisted in replacing the hydrogen first by potassium, and acting upon the compound thus formed by the chloride or iodide of the carburetted hydrogen which was to be introduced in the place of that hydrogen. I commenced with common alcohol, which, after careful purification, was saturated with potassium, and as soon as the action had ceased, mixed with a portion of iodide of æthyle equivalent to the potassium used. Iodide of potassium was readily formed on the application of a gentle heat, and the desired substitution was effected; but, to my astonishment, the compound thus formed had none of the properties of an alcohol—it was nothing else than common æther, $C^4 H^{10} O$.

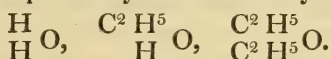
Now this result at once struck me as being inconsistent with the higher formula of alcohol; for if that body contained twice as many atoms of oxygen as are in æther, I ought clearly

* Communicated by the Author; having been read before the British Association at Edinburgh, August 3, 1850.

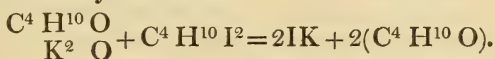
to have obtained a product containing twice as much oxygen as æther does. The alternative was evident; for having obtained æther by substituting $C^2 H^5$ for H in alcohol, the relative composition of the two bodies is represented *by expressing that fact in our formula*. Thus alcohol is $\begin{smallmatrix} C^2 H^5 \\ H \end{smallmatrix} O$, and the potassium compound is $\begin{smallmatrix} C^2 H^5 \\ K \end{smallmatrix} O$; and by acting upon this by iodide of æthyle, we have



Of course the proportion between the two bodies is the only point upon which I here enter, and the same reasoning would be applicable to any multiple of the formulæ assumed. Some chemists may perhaps prefer doubling them in order to avoid the use of atoms of hydrogen, potassium, &c.; but I have not felt myself justified in doing so, because that would involve doubling the usual formula for water; for, as I will presently show, water is formed in ætherification by replacing the carburetted hydrogen of alcohol by hydrogen, which, of course, obliges us to assume the same unity of oxygen in both. Alcohol is therefore water in which half the hydrogen is replaced by carburetted hydrogen, and æther is water in which both atoms of hydrogen are replaced by carburetted hydrogen: thus,



This formation of æther might however be explained after a fashion by the other theory—by supposing the potassium compound to contain æther and potash, which separate during the action of the iodide of æthyle; so that half the æther obtained would have been contained in that compound, and the other half formed by double decomposition between potash and iodide of æthyle: thus—



But although the insufficiency of this explanation becomes evident on a little reflection, I devised a further and more tangible method of arriving at a conclusion. It consisted in acting upon the potassium compound by iodide of methyle, in which case I should, if that compound were æther and potash, obtain a mixture of æther and oxide of methyle; whereas in the contrary case I should obtain a body of the composition $C^3 H^8 O$. Now this substance I obtained, and neither æther nor oxide of methyle.

In this experiment the two theories cross one another, and must lead to different results; for it is evident that, in the first-mentioned decomposition by which æther was formed, the only difficulty in explaining the process decisively consisted in our inability to prove that the carburetted hydrogen introduced instead of the hydrogen did not have in the product an atom of oxygen to itself, but that, on the contrary, it was coupled with the carburetted hydrogen already contained in the alcohol—the two in combination with one atom of oxygen. It is clear that if alcohol *contain* æther and water, and the carburetted hydrogen in my first experiment formed a second atom of æther by taking the place of the hydrogen of this water, that the process being the same in the second experiment, we should then have obtained two æthers. Whereas if the formation of æther from alcohol be effected by synthesis, a new carburetted hydrogen being added to the one already contained in the alcohol, we ought to obtain the new intermediate æther which I obtained.

The complete description of this remarkable body, and of its decompositions, will form the subject of a future paper. I will now merely state that its boiling-point is a little above 10° Cent.; it is possessed of a very peculiar smell, distinctly different from that of common æther; and, like that body, it is only slightly soluble in water. It is not acted upon by the alkali-metals at the common atmospheric temperature.

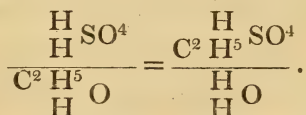
By acting upon the potassium-alcohol in like manner by iodide of amyle, I effected a similar substitution of the elements of that carburetted hydrogen in the place of the hydrogen of alcohol, and obtained an æther boiling at 111° C., having the composition $C^7 H^{16} O$. There is some reason to believe that this body is the same which Balard obtained by decomposition of chloride of amyle by an alcoholic solution of hydrated potash, and which that distinguished chemist took for oxide of amyle.

From the perfect analogy of properties between the known terms of the alcoholic series, it was to be expected that similar substitutions might be effected in the others; and I have verified this by experiment. Of course the formulæ of the other alcohols must be reduced to half, for the same reasons as that of common alcohol. Methylic alcohol is therefore expressed by the formula $\begin{smallmatrix} C^1 H^3 \\ H \end{smallmatrix} O$, as common alcohol is $\begin{smallmatrix} C^2 H^5 \\ H \end{smallmatrix} O$; and in the same manner amylic alcohol is $\begin{smallmatrix} C^5 H^{11} \\ H \end{smallmatrix} O$, and the same of the higher ones. In conformity to this fact, we must be able to obtain the same intermediate æthers by replacing hydrogen in these alcohols (methylic and amylic) by the carbu-

retted hydrogen of iodide of æthyle, as by the inverse process described above. This I have verified in the case of the three-carbon æther, which may be obtained indifferently by replacing one-fourth of the hydrogen of methylic alcohol by $C^2 H^5$, or by replacing one-sixth of the hydrogen of common alcohol by CH^3 . Its rational formula is therefore $\frac{C^2 H^5}{C H^3} O$.

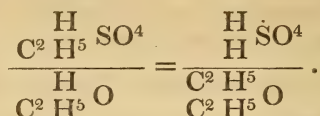
By acting upon the compound $\frac{C H^3}{K} O$ by iodide of amyle, I obtained a third æthereal compound, of which the formula is $\frac{C H^3}{C^5 H^{11}} O$. This is evidently the only one of the three new æthers, which, containing an even number of carbon atoms, might be conceived to have been formed from one alcohol; but when treated with monobasic acids, as hydrochloric, it cannot be expected to act in the same manner as its homogeneous isomeric, the æther $\frac{C^3 H^7}{C^3 H^7} O$ of the three-carbon alcohol $\frac{C^3 H^7}{H} O$; but of this I will give an exact account in the paper above alluded to.

My task is now to explain the process of ætherification by the action of sulphuric acid ($SO^4 H^2$) upon alcohol; and in order to accomplish that, I must show the connexion between those substances and the reagents used in the above-described experiments. With this view, I have merely to add to the above facts the acknowledged analogy of the simple and compound radicals in their compounds. I must first show how a substance analogous to my iodide of æthyle is formed, and then how by double decomposition with alcohol it produces æther. This is very easy; for sulphovinic acid is strictly analogous to iodide of æthyle plus iodide of hydrogen, which we should obtain by replacing SO^4 in its formula by an equivalent of iodine; and in order to represent the formation of this sulphovinic acid, which is well known to precede that of æther, the simplest mode is at the same time the one most free from hypothesis; it consists in stating the fact, that sulphuric acid and alcohol are transformed into sulphovinic acid and water, by half the hydrogen of the former changing places with the carburetted hydrogen of the latter: thus—



Now from this point it is clear that the process is the same as in the decompositions above described; for by this sulphovinic

acid coming in contact with an atom of alcohol, it reacts exactly in the same manner as the iodide did, forming of course sulphuric acid and æther :



The sulphuric acid thus reproduced comes again in contact with alcohol, forming sulphovinic acid, which reacts as before ; and so the process goes on continuously, as found in practice.

We thus see that the formation of æther from alcohol is neither a process of simple separation, nor one of mere synthesis ; but that it consists in the substitution of one molecule for another, and is effected by double decomposition between two compounds. I therefore admit the contact theory, inasmuch as I acknowledge the circumstance of contact as a necessary *condition* of the reaction of the molecules upon one another. By reducing the formulæ of the alcohols to one atom of oxygen, I also retain the equality of volumes which the contact theory insists upon between the vapours of these bodies and their æthers, so that æther truly contains the elements of olefiant gas in addition to those of alcohol in one atom. But, on the other hand, I attach equal importance to all the essential facts of the chemical theory, and rest my explanation of the process as much upon them as upon those of the contact theory ; for, one-sixth of the hydrogen in alcohol truly exhibits different reactions from the remaining five, and must therefore be contained in that compound in a different manner from them ; and the alternate formation and decomposition of sulphovinic acid is to me, as to the partisans of the chemical theory, the key to explaining the process of ætherification.

Innovations in science frequently gain ground *only* by displacing the conceptions which preceded them, and which served more or less directly as their foundation ; but, if the view which I have here presented be considered a step in our understanding of the subject, I must beg leave to disclaim for it the title of innovation ; for my conclusion consists in establishing the connexion and showing the compatibility of views which have hitherto been considered contrary ; and the best possible justification of the eminent philosophers who advocated either one of the two contending theories, is thus afforded by my reconciling their arguments with those of their equally illustrious opponents.

Before quitting the subject of ætherification, I would wish to add a few words on an application which naturally enough

suggests itself of the fact to which the process is here ascribed. I refer to the transfer of homologous molecules in alternately opposite directions, which, as I have endeavoured to show, is the cause of the continuous action of sulphuric acid in this remarkable process. It may naturally be asked, why do hydrogen and carburetted hydrogen thus continuously change places? It cannot be from any such circumstance as superior affinity of one molecule over another, for one moment sees reversed with a new molecule the transfer effected during the preceding one. Now in reflecting upon this remarkable fact, it strikes the mind at once that the facility of interchange must be greater the more close the analogy between the molecules exchanged; that if hydrogen and amyle can replace one another in a compound, hydrogen and æthyle, which are more nearly allied in composition and properties, must be able to replace one another more easily in the same compound; and that the facility of interchange of hydrogen and methyle, which are still more similar, will be still greater. But if this be true, must not the exchange of one molecule for another of *identical* properties be the most easily effected of all? Surely it must, if there be any difference at all; and if so, the law of analogy forbids our imagining the fact to be peculiar to hydrogen among substances resembling it in other respects. We are thus forced to admit, that, in an aggregate of molecules of any compound, there is an exchange constantly going on between the elements which are contained in it. For instance, a drop of hydrochloric acid being supposed to be made up of a great number of molecules of the composition Cl H , the proposition at which we have just arrived would lead us to believe that each atom of hydrogen does not remain quietly in juxtaposition with the atom of chlorine with which it first united, but, on the contrary, is constantly changing places with other atoms of hydrogen, or, what is the same thing, changing chlorine. Of course this change is not directly sensible to us, because one atom of hydrochloric acid is like another; but suppose we mix with the hydrochloric acid some sulphate of copper (of which the component atoms are undergoing a similar change of place), the basulous elements hydrogen and copper do not limit their change of place to the circle of the atoms with which they were at first combined, the hydrogen does not merely move from one atom of chlorine to another, but in its turn also replaces an atom of copper, forming chloride of copper and sulphuric acid. Thus it is, that at any moment of time in which we examine the mixture, the bases are divided between the acids; and in certain cases, where the difference of properties of the analogous molecules

is very great, it is found that the stronger acid and stronger base remain almost entirely together, leaving the weaker ones combined. This is well known in the case of a mixture of sulphuric acid and borax, and is a confirmation of our fundamental assumption, that the greater the difference of properties, the more difficult is the alternate interchange of one molecule for another.

But suppose now that instead of sulphate of copper, we mixed sulphate of silver with our hydrochloric acid in aqueous solution, and that a similar division of the bases between the acids established itself in the first moment, forming four compounds, SO^4H^2 , SO^4Ag^2 , ClH , ClAg ; it is clear that this last-mentioned compound, being insoluble in water, must, on its formation, separate out and remove from the circle of decompositions which solubility established. But of course the three compounds remaining in solution continue the exchange of their component parts, and give rise successively to new portions of chloride of silver, until as much of that compound is precipitated as the liquid contained equivalents of its component parts, a very small quantity remaining in solution and in the circle of decompositions.

Such is the general process of chemical decomposition. Of course a compound is removed as effectually from the circle of decompositions by possessing the gaseous form under the circumstances of the experiment, or even by being a liquid insoluble in the menstruum. I believe this explanation coincides in its second part with the one proposed many years ago by Berthollet; but not making use of the atomic hypothesis, upon which my explanation is based, that eminent philosopher went no farther back than the division of the acids between the bases on the mixture of salts, a fact which I have here deduced from the motion of atoms. It is well known that the general fact upon which Berthollet founded his view is denied by some eminent chemists of the present day; but I believe the instances which they adduce are only apparent exceptions to the law, and will on further examination be found to afford additional confirmation of the truth of the great Savoyrien's conception, as I have shown in the case of boracic and sulphuric acids.

In using the atomic theory, chemists have added to it of late years an unsafe, and, as I think, an unwarrantable hypothesis, namely that the atoms are in a state of rest. Now this hypothesis I discard, and reason upon the broader basis of atomic motion.

XLVI. *Account of a remarkable Meteor, seen December 19, 1849. By Professor J. D. FORBES*.*

ON the evening of the 19th December 1849, whilst walking near the southern part of Edinburgh, about fifteen minutes past five, Greenwich time (as I afterwards estimated), I observed a meteor, fully brighter than Venus at her average brilliancy, moving from W. towards N., parallel to the horizon, elevated 15° above it, and followed by a distinct luminous train. This angle was subsequently taken by estimation by daylight, with the aid of a theodolite; and the compass-bearing of the meteor, when first seen, ascertained in the same way, must have been 47° W. of N. When it bore 29° E. of magnetic north, it was observed to have divided into two, the one part following the other at some distance; and I soon after lost sight of it in the obscurity of the smoke of the town. When it split, its altitude was estimated at 6° . It thus described an arc of no less than 76° , in doing which it occupied, as I roughly estimated, about fifteen seconds, or possibly more.

Having sent a short notice of the appearance of the meteor to the *Courant* newspaper, I received from many quarters accounts of its having been seen under circumstances remarkably similar to those just described. I believe that nearly forty communications on the subject have reached me from places included between Longford, in the centre of Ireland, to near Bervie in Kincardineshire, a distance of above 300 miles, in a direction nearly N.E. and S.W.; whilst in a perpendicular direction, or from N.W. to S.E., the range of observation has been comparatively small; for I have received no information from beyond Renfrew in the one direction, and Durham in the other, being about 140 miles distant in a straight line. The meteor was seen at Longford, in Ireland, 74 miles west of Dublin, but not in Dublin itself. It was seen at Belfast, between Carlisle and Gretna at Stewarton in Ayrshire, at Johnstone, at Paisley, Renfrew, and by many persons in Glasgow and the neighbourhood. It was also generally seen in Edinburgh, in East Lothian, near Melrose, and at Durham, as already mentioned. Further north, I have received accounts from Crail, St. Andrews, Dundee, Perth, and Johnshaven to the north of Montrose.

The greater number of these communications concur in estimating the direction of the motion of the meteor to have been from S.W. to N.E., although, as might be expected,

* From the Proceedings of the Royal Society of Edinburgh, vol. ii. No. 39.

they vary excessively as to its distance and magnitude; being described by some persons as only 50 or 100 yards off, and as large as the moon; by others, as a ball of 9 inches in diameter, or the size of a large egg. One person only professes to have heard a sound. The time during which it was seen was variously estimated. At Longford, by Mr. Curtis, 20 seconds; at Glasgow, by Mr. Stevenson, at 20 seconds; at Johnstone, by Mr. Cunningham, 15 seconds; at Perth, 15 or 20 seconds; at Durham, by Mr. Carrington, 30 seconds; at St. Andrews, 15 seconds according to one observer, and 18 to 21 seconds according to another; at Johnshaven, $\frac{3}{4}$ ths of a minute. The hour of the appearance of the meteor, in most of the descriptions, is stated at between 5^h 10^m and 5^h 16^m.

The arc of the horizon which it was seen to traverse depended, of course, on the point where the meteor first caught the observer's eye. At Granton, it was traced by Professor Kelland through 125° of azimuth; at Perth, 130°; at St. Andrews, 74°; at Edinburgh, 76°; at Durham, 65°; at Glasgow, from 60° to 70°. The division of the head or nucleus into several parts, and, first of all (in most cases), into *two*, has been noticed with remarkably slight variation; consequently, the explosion of the meteor marks a well-determined point in its path. The separation was specially noticed at Edinburgh, Granton, Glasgow, Renfrew, Melrose, Haddington, Johnshaven, Perth, Durham and St. Andrews.

In a majority of cases a luminous train was observed; and I am confident that the existence of this train, which has been estimated at from 2° to 3° long, cannot be questioned. Dr. Adamson, however, especially remarked that no train was to be seen at St. Andrews.

On revising the whole accounts, it does not appear that any of them can be relied upon for ascertaining the position of the meteor in space, except the observations of Mr. Carrington of the Durham observatory; of Professor Kelland, Mr. Stirling and myself, at Edinburgh; of Dr. Adamson and another observer, communicated by Professor Fischer of St. Andrews; of a young gentleman at Perth, communicated by Thomas Miller, Esq., Rector of the Perth Academy; and of A. D. Stevenson, Esq., and W. Gourlie, Esq., jun., at Glasgow. My inquiries were chiefly directed to the two following points: *first*, the angular elevation of the meteor in the N.W. quarter of the heavens, where it is admitted by all that its path appeared almost horizontal; *secondly*, to the bearing of the meteor at the instant of explosion.

At Durham, Mr. Carrington saw the meteor first when the bearing was true N.W., the altitude (by theodolite) was then-

10° , or not exceeding 11° ; when it burst it was due N. (true), and continued to move 10° or 12° further before it disappeared. Professor Chevallier, who obligingly communicated these results, states that the meteor appeared rather to rise as it approached the north, but with a doubt. This supposition, however, appears inadmissible, from the unanimity of the other accounts.

At Granton, near Edinburgh, Professor Kelland caught sight of the meteor a little to the N. of the moon, and several diameters below it. This corresponds, by after estimation with a theodolite, to 75° W. of magnetic N., and an altitude of 12° . Professor Kelland thinks that it rather rose afterwards. It split into two at 20° E. of magnetic N., having then an altitude of only 5° ; it continued for a considerable time bright, then began to fade, as if by the effect of distance, and also to separate into several parts: it was finally lost sight of 50° E. of magnetic N. (this bearing is well ascertained), with an altitude estimated at only half a degree. The position and circumstances of these observations, made at an elevated station above the Frith of Forth, were eminently favourable.

Mr. J. Stirling, civil engineer, looking up North Hanover Street, Edinburgh, saw the meteor separate into two parts; the bearing he afterwards estimated at 25° E. of magnetic N. (the probable error not exceeding 1°), and the altitude at $8^{\circ} 30'$, certainly not exceeding 9° .

I think we may conclude, that at Edinburgh the meteor attained a maximum elevation of 15° (that mentioned in the commencement of this paper), since it no doubt rose after Professor Kelland first saw it to the S. of the true W., with an altitude of only 12° . The course of the meteor was evidently such as to be nearest the spectator when in the true N.W. or W.N.W.

The place of the meteor when it burst stands thus:—

Kelland, N. 20° E. (mag.)	Alt. 5° .
Stirling, N. 25° E.	Alt. $8^{\circ} 30'$.
Forbes, N. 29° E.	Alt. 6° .

The average is almost 25° E. of N., or about 1° W. of the true meridian, the variation being nearly 26° . The mean of the three observations of altitude would be $6^{\circ} 30'$; but admitting Mr. Stirling's to be entitled to the greatest confidence, we may suppose it 7° , or possibly a little more.

At St. Andrews, the meteor was seen by Dr. Adamson, when riding in a northerly direction on the Largo road. Professor Fischer was so kind as to accompany him afterwards to the spot, and to reduce his observations with all the accuracy

of which they were capable. It was first noticed when bearing $8\frac{1}{2}^{\circ}$ W. of magnetic N., and disappeared at $42\frac{1}{2}^{\circ}$ E. of N.; the altitude was conjecturally stated as between 14° and $18\frac{1}{2}^{\circ}$, and it appeared to move horizontally, but rather declining towards the N.

After describing three-fourths of its course, it split into two parts, which went on close together for a little, then broke into four or five, became dull red, and rapidly disappeared; the separate pieces travelling on together until the last.

Another intelligent observer near St. Andrews, whose evidence was taken by Mr. Fischer, first saw the meteor $29\frac{3}{4}^{\circ}$ W. of magnetic N., and estimated the point where the meteor burst at 44° E. of N; but this last number coincides so closely with Dr. Adamson's estimate of the point of final disappearance, that it is perhaps allowable to suppose, that this second observer had mixed up these two events in his description. Dr. Adamson's statement, that one-fourth of the arc which he saw was described after the meteor had split, would give an azimuth at that moment of almost 30° E. of N. magnetic, or 4° E. of N. true, as Mr. Fischer determined the magnetic declination to be about $25^{\circ} 46'$. The altitude of the meteor, as seen by this observer, appears not to have exceeded 15° (the same as at Edinburgh); which number we shall therefore adopt.

At Perth, the passage of the meteor was seen from the North Inch, by a young gentleman of intelligence, whose observations were reduced to numbers by Mr. Miller, Rector of the Perth Academy, who was so good as to accompany him to the spot, and take the angles with a theodolite. Its bearing, when first seen, was 46° S. of W. true; its angular altitude was at that time only $3^{\circ} 30'$. This is by far the most southern azimuth which has been observed. Its bearing, when it disappeared, was 6° W. of N., but it was then lost in a cloud. If I understand right, it had by this time separated into fragments. Its apparent altitude in the middle of its course was about $17^{\circ} 30'$. These observations, extending over an arc of 130° , taken along with Professor Kelland's, clearly demonstrate that the meteor appeared with a very low altitude in the S.W. quarter of the heavens, and disappeared in a similar way in the N.N.E., attaining its greatest elevation about W.N.W. (true).

At Glasgow the meteor was very generally and well seen. Mr. William Gourlie, jun., saw it move from S.W. to N.N.E., over an arc of 60° or 70° , and divide into two, when it bore 40° E. of magnetic N. He estimates its greatest elevation at 30° ; and that it decreased to between 15° and 17° , or even

less at the time of its separation: he adds, that he is not much accustomed to such observations. Mr. A. D. Stevenson, living in South Portland Street, Glasgow, saw the meteor moving along at a height just sufficient to clear the chimney-tops, on the west side of the street; an elevation which he afterwards estimated, as he states, with considerable accuracy at 28° . I have received further and more minute accounts of the appearance of the meteor from Mr. Stevenson, who has been most kind and intelligent in his communications; and my friend Mr. James Peddie has verified the accuracy of Mr. Stevenson's observations beyond the possibility of mistake. It appears that the meteor passed quite clear of a stack of chimneys on the opposite side of the street, which would give it a well-defined minimum altitude of $25^{\circ} 41'$; but Mr. Stevenson is of opinion that it rose more than 2° higher, or to not less than 28° (perhaps even to $28^{\circ} 21'$); when it was highest, its bearing was $52\frac{1}{2}^{\circ}$ W. of N. (magnetic), and it disappeared from his view when it bore $40^{\circ} 27'$ E. of magnetic N. *It was then decidedly single.* Now this bearing coincides with that at which Mr. Gourlie observed it to become *double*, and consequently the limit towards the N. of this event is severely defined.

The following table contains the most definite of these observations, and the azimuths are all reduced to the true meridian.

	Greatest altitude.	True azimuth when first seen.	True azimuth of disappearance.	Arc observed.	True azimuth of first explosion.	Altitude at first explosion.
Durham ...	$10^{\circ} 30'$	N. 45° W.	N. 12° E.	57°	N.	
Edinburgh	15	W. 11° S.	N. 24° E.	125	N. 1° W.	7°
St. Andrews	15	N. 55° W.	N. 16° E.	71	N. 4° E.	
Perth	$17^{\circ} 30'$	W. 47° S.	N. 7° W. (in a cloud)	130	?	
Glasgow ...	28			100° ?	N. 14° E.	15

Remarks on the Observations.

1. On the whole, these observations are not consistent, and cannot (I conceive) be cleared up without additional and accurate ones, which it may now be too late to procure. The central group of stations, Edinburgh, Perth and St. Andrews, are sufficiently accordant, and indicate that the path of the meteor must have been nearly parallel to a line passing through the first and last of those places, or in a direction N. 27° E. (true); which accords well with the observations at most of the individual stations, and particularly with the *va-*

nishing direction in Professor Kelland's remarkable observation at Granton.

2. The Durham observation is compatible with the above-mentioned group within the limits of error. By the combination of Durham and Edinburgh (the base line perpendicular to the assumed direction of the meteor's motion being ninety-five miles), I calculated that the meteor passed vertically nearly over the Island of St. Kilda, with an absolute elevation of about eighty-eight miles. But this solution seems absolutely excluded by observations at Glasgow which admit of no question, and which I have spared no pains in verifying. Had the position of the meteor been such as I have first assumed, it could not possibly have been seen over even the roofs of the houses from the station occupied by Mr. Stevenson, much less over the chimney-tops. The bearing at the moment of explosion at Glasgow, also singularly enough corroborates sufficiently well the comparatively small elevation (about twenty miles above the earth) which the combination of Edinburgh and Glasgow gives; and this bearing we have seen to have been also accurately defined by the physical obstacles bounding the observer's view; it would have given a parallax of 15° , subtended by the perpendicular on the meteor's path, referred to Glasgow and Edinburgh respectively. Now, if this calculation were anything like correct, the Perth observation is entirely wrong; and the meteor could not have risen about 6° above the horizon of Durham, instead of 10° or 11° as estimated. I am unable in any degree to explain these conflicting results.

3. The observations of Professor Kelland at Granton, and those at Perth, through the great azimuths of 125° and 130° , described by the meteor with such remarkable deliberation of motion, lead, when analysed, to the very same results which presented themselves to the mind of the spectator intuitively; namely, that the motion must have been sensibly rectilinear, equable, and parallel to the horizon at Edinburgh. Assuming that the greatest altitude at Edinburgh was 15° , and the bearing then N. 63° W. (true), we may calculate that the altitude should have been on this hypothesis, when first seen by Professor Kelland, $11^\circ 47'$, instead of 12° as observed; at explosion, $6^\circ 59'$ (7° observed), and at its final disappearance $0^\circ 47'$ (instead of $0^\circ 30'$ observed). Again, at Perth, the observed altitude, when first seen, was $3\frac{1}{2}^\circ$, and the calculated altitude $5^\circ 3'$, taking the maximum altitude at $17\frac{1}{2}^\circ$. The coincidence is, on the whole, remarkable; though it would be rash to push it to an extreme, as an error of some degrees may exist in the assumption of the direction of the meteor's

course. Some later observations, received from Mr. Curtis at Longford, and a consideration of the effects of perspective at Perth and Edinburgh, incline me to admit that the path might make an angle 3° or 4° greater with the meridian than I have above supposed. These conclusions are independent of the actual distance or parallax of the meteor; which, as I have said, cannot be determined without further observations, which I should be glad to receive from any quarter, but more particularly from Ireland, and from the centre and N.W. of Scotland. If correct, they entitle us to infer that the meteor in question was most probably a body moving in space, in a path little curved, and not revolving round the earth.

XLVII. *Additions to the articles in the September Number of this Journal, "On a new Class of Theorems," and on Pascal's Theorem.* By J. J. SYLVESTER, M.A., F.R.S.*

FIRST addition.—I have alluded in the above article to a more general theorem, comprising, as a particular case, the theorem there given for the simultaneous evanescence of two quadratic functions of $2n$ letters, or n linear equations becoming instituted between the letters.

In order to make this generalization intelligible, I must premise a few words on the Theory of Orders, a term which I have invented with particular reference to quadratic functions, although obviously admitting of a more extended application. A linear function of all the letters entering into a function or system of functions under consideration I call an order of the letters, or simply an Order. Now it is clear that we may always consider a function of any number of letters as a function of as many orders as there are letters; but in certain cases a function may be expressed in terms of a fewer number of orders than it has letters, as when the general characteristic function of a conic becomes that of a pair of crossing lines or a pair of coincident lines, in which event it loses respectively one and two orders, and so for the characteristic of a conoid becoming that of a cone, a pair of planes or two coincident planes, in which several events, a function of four letters, becomes that of only three orders, or two orders, or one order, respectively. When a function may be expressed by means of r orders less than it contains letters, I call it a function minus r orders. I now proceed to state my theorem.

Let U and V be functions each of the same m letters, and

* Communicated by the Author.

suppose that the determinant in respect of those letters of $U + \mu V$ contains i pairs of equal linear factors of μ ; then it is possible, by means of i linear equations instituted between the letters, to make U and V each become functions of *the same* $m - 2i$ orders; and conversely, if by i equations between the letters U and V may be made functions of *the same* $m - 2i$ orders, the determinant of $U + \mu V$ considered as a function of μ will contain i quadratic factors.

Thus when $m = 2n$ and $i = n$, U and V will each become functions of zero orders, *i. e.* will both disappear, provided that on the institution of a certain system of n linear equations, among the letters of which U and V are functions, the determinant of $(U + \mu V)$ is a perfect square,—which is the theorem given in the article referred to.

So (*ex. gr.*) if U and V be quadratic functions of four letters, and therefore the characteristics of two conoids, $|\square| (U + \mu V)$ being a perfect square, expresses that these conoids have a straight line in common lying upon each of their surfaces.

If U and V be quadratic functions of three letters only, and admit therefore of being considered as the characteristics of two conics, $|\square| (U + \mu V)$ containing a square factor, is indicative of these conics having a common tangent at a common point, *i. e.* of their touching each other at some point; for it is easily shown that the disappearance of two orders from any quadratic function by virtue of one linear function of its letters being zero, indicates that the line, plane, &c. of which the linear function is the characteristic is a tangent to the curve, surface, &c. of which the quadratic function is the characteristic.

I pass now to a generalization of the theorem which shows how to express, under the form of a double determinant, the resultant of one linear and two quadratic homogeneous functions of three letters (which I should have given in the original paper, had I not there been more intent upon developing an ascending scale than of expatiating upon a superficial ramification of analogies), and which constitutes my *Second addition* to that paper, to wit—

If U and V be homogeneous quadratic, and $L_1 L_2 \dots L_n$ homogeneous linear functions of $(n + 2)$ letters $x_1 x_2 \dots x_{n+2}$, the determinant of the entire system of $n + 2$ functions is equal to

$$\begin{vmatrix} \square & \square \\ \lambda, \mu & x_1, x_2, \dots x_{n+2} t_1 t_2 \dots t_n \end{vmatrix} \{ \lambda U + \mu V + L_1 t_1 + L_2 t_2 + \dots + L_n t_n \};$$

the demonstration is precisely similar to the analytical one

given in the September Number for the particular case of $n=1$.

When $n=0$, we revert to Mr. Boole's theorem of elimination between U and V already adverted to. The proof, it will be easily recognized, does not require the application of the more general theorem relative to the simultaneous depression of orders of two quadratic functions, but only the limited one before given, which supplies the conditions of their simultaneous disparition. I now proceed to develop more particularly certain analogies between the theory of the mutual contacts of two conics, and that of the tangencies to the intersection of two conoids.

But here again I must anticipate some of the results which will be given in my forthcoming memoir on Determinants and Quadratic Functions, by explaining what is to be understood by minor determinants, and the relation in which they stand to the complete determinant in which they are included. This preliminary explanation, and the statement of the analogies above alluded to, will constitute my *Third* and last addition.

Imagine any determinant set out under the form of a square array of terms. This square may be considered as divisible into lines and columns. Now conceive any one line and any one column to be struck out, we get in this way a square, one term less in breadth and depth than the original square; and by varying in every possible manner the selection of the line and column excluded, we obtain, supposing the original square to consist of n lines and n columns, n^2 such minor squares, each of which will represent what I term a First Minor Determinant relative to the principal or complete determinant. Now suppose two lines and two columns struck out from the

original square, we shall obtain a system of $\left\{ \frac{n(n-1)}{2} \right\}^2$ squares, each two terms lower than the principal square, and representing a determinant of one lower order than those above referred to. These constitute what I term a system of Second Minor Determinants; and so in general we can form a system of r th minor determinants by the exclusion of r lines and r columns, and such system *in general* will contain

$$\left\{ \frac{n \cdot (n-1) \dots (n-r+1)}{1 \cdot 2 \dots r} \right\}^2$$

distinct determinants.

I say '*in general*;' because if the principal determinant be totally or partially symmetrical in respect to either or each of its diagonals, the number of distinct determinants appertaining to each system of minors will undergo a material diminution, which is easily calculable.

Now I have established the following law:—

The whole of a system of r th minors being zero, implies only $(r+1)^2$ equations, *i. e.* by making $(r+1)^2$ of these minors zero, all will become zero; and this is true, no matter what may be the dimensions or form of the complete determinant. But furthermore, if the complete determinant be formed from a quadratic function, so as to be symmetrical about one of its diagonals, then $\frac{(r+1)(r+2)}{2}$ only of the r th minors being zero, will serve to imply that all these minors are zero. Of course, in applying these theorems, care must be taken that the $(r+1)^2$ or $\frac{(r+1)(r+2)}{2}$ selected equations must be mutually non-implicative, and shall constitute independent conditions.

In the application I am about to make of these principles, we shall have only to deal with a system of *first* minors and of a *symmetrical* determinant. If three of these properly selected be zero, from the foregoing it appears that all must be zero.

Now let U and V be characteristics of two conics, *i. e.* let each be a function of only three letters, it may be shown (see my paper in the Cambridge and Dublin Mathematical Journal for November 1850) that the different species of contacts between these two conics will correspond to peculiar properties of the compound characteristic $U + \mu V$.

If the determinant of this function have two equal roots, the conics simply touch; if it have three equal roots, the conics have a single contact of a higher order, *i. e.* the same curvature; if its six first minors all become zero simultaneously for the same value of μ , the conics have a double contact. If the same value of μ , which makes all these first minors zero, be at the same time not merely a double root (as of analytical necessity it always must be), but a treble root of

$$|\square| (U + \mu V) = 0,$$

then the conics have a single contact of the highest possible order short of absolute coincidence, *i. e.* they meet in four consecutive points.

The parallelism between this theory and that of two quadratic functions P , Q , and one linear function L^* of four letters, say x, y, z, t , is exact†. For let $P + Lu + \mu Q$ be now taken

* Observe that $P=0$, $Q=0$, $L=0$ now express the equations to two conoids and a plane respectively.

† This parallelism may be easily shown analytically to imply, and be implied, in the geometrical fact, that the contact of the plane L with the intersection of the two surfaces P and Q , is of exactly the same kind as the contact (which must exist) between the two conics which are the intersections of P and Q respectively with the plane L .

as our compound characteristic (a function, it will be observed, of five letters, x, y, z, t, u) ; if its determinant have two equal roots, L has two consecutive points in common with the intersection of P and Q , *i. e.* passes through a tangent to that intersection ; if it have three equal roots, L has three consecutive points in common with the said intersection, *i. e.* is an osculating plane thereto ; if its fifteen first minors admit of all being made simultaneously zero, L has a double contact with the intersection of P and Q , *i. e.* it is a tangent plane to some one of the four cones of the second order containing this intersection ; if the same linear function of μ which enters into all these first minors be contained cubically in the complete determinant, then the plane L passes through four consecutive points of the intersection of P and Q , and the points where it meets the curve will be points of contrary plane flexure ; and, as it seems to me, at such points the tangential direction of the curve must point to the summit of one or the other of the four cones above alluded to*. In assigning the conditions for L being a double tangent plane to the intersection of P and Q , we may take any three independent minors at pleasure equal to zero. One of these may be selected so as to be clear of the coefficients of L ; in fact, the determinant of $P + \mu Q$ will be a first minor of $P + \mu Q + Lu$; μ may thus be determined by a biquadratic equation ; and then, by properly selecting the two other minors, we may obtain two equations in which only the first powers of the coefficients of x, y, z, t in L appear, and may consequently obtain L under the form of

$$(ae + \alpha)x + (be + \beta)y + (ce + \gamma)z + (de + \delta)t,$$

where $a, \alpha ; b, \beta ; c, \gamma ; d, \delta$ will be known functions of any one of the four values of μ . The point of contact being given will then serve to determine e , and we shall thus have the equation to each of the four double tangent planes at any given point fully determined.

In the foregoing discussions I have freely employed the word *characteristic* without previously defining its meaning, trusting to that being apparent from the mode of its use. It is a term of exceeding value for its significance and brevity. The characteristic of a geometrical figure † is the function which,

* If this be so, then we have the following geometrical theorem :—“ *The summit of one of the four cones of the second degree which contain the intersections of two surfaces of the second order drawn in any manner respectively through two given conics lying in the same plane, and having with one another a contact of the third degree, will always be found in the same right line, namely in the tangent line to the two given conics at the point of contact.*”

† More generally, the *characteristic* of any fact or existence is the func-

equaled to zero, constitutes the equation to such figure. Plücker, I think, somewhere calls it the line or surface function, as the case may be. Geometry, analytically considered, resolves itself into a system of rules for the construction and interpretation of characteristics. One more remark, and I have done. A very comprehensive theorem has been given at the commencement of this commentary, for interpreting the effect of a complete determinant of a linear function of two quadratic functions ($U + \mu V$), having one or more pairs of equal factors ($e + \epsilon\mu$). But here a far wider theory presents itself, of which the aim should be to determine the effect and meaning of this determinant, having any amount and distribution of multiplicity whatsoever among its roots. Nor must our investigations end at that point; but we must be able to determine the meaning and effect of common factors, one or more entering into the successive systems of *minor* determinants derived from the complete determinant of $U + \mu V$.

Nor are we necessarily confined to two, but may take several quadratic functions simultaneously into account.

Aspiring to these wide generalizations, the analysis of quadratic functions soars to a pitch from whence it may look proudly down on the feeble and vain attempts of geometry proper to rise to its level or to emulate it in its flights.

26 Lincoln's-Inn-Fields,
September 3, 1850.

The law which I have stated for assigning the number of independent, or to speak more accurately, non-coevanescent determinants belonging to a given system of minors, I call the Homaloidal law, because it is a corollary to a proposition which represents analytically the indefinite extension of a property common to lines and surfaces to all loci (whether in ordinary or transcendental space) of the first order, all of which loci may, by an abstraction derived from the idea of levelness common to straight lines and planes, be called Homaloids. The property in question is, that neither two straight lines nor two planes can have a common segment; in

tion which, equaled to zero, expresses the condition of the actuality of such fact or existence.

Perhaps the most important pervading principle of modern analysis, but which has never hitherto been articulately expressed, is *that*, according to which we infer, that when one fact of whatever kind is implied in another, the characteristic of the first must contain as a factor the characteristic of the second; and that when two facts are mutually involved, their characteristics will be powers of the same integral function.

The doctrine of characteristics, applied to dependent *systems* of facts, admits of a wide development, logical and analytical.

other words, if n independent relations of rectilinearity or of coplanarity, as the case may be, exist between triadic groups of a series of $n+2$, or between tetradic groups of a series of $n+3$ points respectively, then every triad or tetrad of the series, according to the respective suppositions made, will be in rectilinear or in plane order. So, too, if n independent relations of *coincidence* exist between the duads formed out of $n+1$ points, every duad will constitute a coincidence.

This homaloidal law has not been stated in the above commentary in its form of greatest generality. For this purpose we must commence, not with a square, but with an oblong arrangement of terms consisting, suppose, of m lines and n columns. This will not in itself represent a determinant, but is, as it were, a Matrix out of which we may form various systems of determinants by fixing upon a number p , and selecting at will p lines and p columns, the squares corresponding to which may be termed determinants of the p th order. We have, then, the following proposition. The number of uncoevanescent determinants constituting a system of the p th order derived from a given matrix, n terms broad and m terms deep, may equal, but can never exceed the number

$$(n-p+1)(m-p+1).$$

Remark on Pascal's and Brianchon's Theorems.

I omitted to state, in the September Number of the Journal, that the demonstration there given by me for Pascal's, applied equally to Brianchon's theorem. This remark is of the more importance, because the fault of the analytical demonstrations hitherto given of these theorems has been, that they make Brianchon's a consequence of Pascal's, instead of causing the two to flow simultaneously from the application of the same principles. No demonstration can be held valid in *method*, or as touching the essence of the subject-matter, in which the indifference of the duadic law is departed from. Until these recent times, the analytic method of geometry, as given by Descartes, had been suffered to go on halting as it were on one foot. To Plücker was reserved the honour of setting it firmly on its two equal supports by supplying the complementary system of coordinates. This invention, however, had become inevitable, after the profound views promulgated by Steiner, in the introduction to his Geometry, had once taken hold of the minds of mathematicians. To make the demonstration in the article referred to apply, *totidem literis*, to Brianchon's theorem (recourse being had to the correlative system of coordinates), it is only needful to consider U as the

characteristic of the tangential envelope of the conic, x, y, z, t, u, v as the characteristics of the six points of the circumscribed hexagon, ϕ the characteristic of the point in which the line x, v meets the line z, t ; $ay - au$ will then be shown to characterize the point in which t, x meets v, z ; and thus we see that $y, u; t, x; v, z$, the three pairs of opposite sides of the hexagon, will meet in one and the same point, which is Brianchon's theorem.

XLVIII. *On the solution of a System of Equations in which three Homogeneous Quadratic Functions of three unknown quantities are respectively equaled to numerical Multiples of a fourth Non-Homogeneous Function of the same.* By J. J. SYLVESTER, M.A., F.R.S.*

LET U, V, W be three homogeneous functions of x, y, z , and let ω be any function of x, y, z of the n th degree, and suppose that there is given for solution the system of equations

$$U = A \cdot \omega$$

$$V = B \cdot \omega$$

$$W = C \cdot \omega.$$

Theorem.—The above system can be solved by the solution of a cubic equation, and an equation of the n th degree.

For let D be the determinant in respect to x, y, z of

$$fU + gV + hW,$$

then D is a cubic function of f, g, h . Now make $D = 0$

$$Af + Bg + Ch = 0,$$

the ratios of $f : g : h$ which satisfy the last two equations can be determined by the solution of a cubic equation, and there will accordingly be three systems of f, g, h which satisfy the same, as

$$\begin{array}{ccc} f_1 & g_1 & h_1 \\ f_2 & g_2 & h_2 \\ f_3 & g_3 & h_3. \end{array}$$

Now $D = 0$ implies that $fU + gV + hW$ breaks up into two linear factors; accordingly we shall find

$$(l_1x + m_1y + x_1z) \cdot (\lambda_1x + \mu_1y + \nu_1z) = 0$$

$$(l_2x + m_2y + x_2z) (\lambda_2x + \mu_2y + \nu_2z) = 0$$

$$(l_3x + m_3y + x_3z) (\lambda_3x + \mu_3y + \nu_3z) = 0,$$

* Communicated by the Author.

in which the several sets of l, m, n ; λ, μ, ν can be expressed without difficulty in terms of the several values of $\sqrt{f}, \sqrt{g}, \sqrt{h}$.

Let the above equations be written under the form

$$PP'=0$$

$$QQ'=0$$

$$RR'=0.$$

Since the given equations are perfectly general, it is readily seen that the equations

$$(P=0 \ P'=0) \ (Q=0 \ Q'=0) \ (R=0 \ R'=0)$$

will severally represent pairs of opposite sides of a quadrangle expressed by general coordinates x, y, z ; so that one of the two functions R, R' will be a linear function of P and Q and also of P' and Q' , and the other will be a linear function of P and Q' and also of P' and Q .*

In order to solve the equations, we need only consider two such pairs as $PP'=0 \ QQ'=0$; we then make

$$P=0 \quad Q=0,$$

or

$$P=0 \quad Q'=0,$$

or

$$P'=0 \quad Q=0,$$

or

$$P'=0 \quad Q'=0.$$

Any one of these four systems will give the ratios of $x : y : z$; and then, by substitution in any one of the given equations, we obtain the values of x, y, z by the solution of an ordinary equation of the n th degree. The number of systems x, y, z is therefore always $4n$.

The equations connected with the solution of Malfatti's celebrated problem, "In a given triangle to inscribe three circles such that each circle touches the remaining two circles and also two sides of the triangle," given by Mr. Cayley in the November Number for 1849 of the Cambridge and Dublin Mathematical Journal, to wit,

$$by^2 + cz^2 + 2fyz = \theta^2. a(bc - f^2) = A$$

$$cz^2 + ax^2 + 2gzx = \theta^2. b(ca - g^2) = B$$

$$ax^2 + by^2 + 2hxy = \theta^2. c(ab - h^2) = C,$$

* Were it not for this being the case, the number of solutions would be n times the number of ways of obtaining duads out of three sets of two things, excluding the duads forming the sets, *i. e.* the number of solutions would be $12n$ in place of $4n$, the true number.

come under the general form which has just been solved. It so happens, however, that in this particular case

$$\left. \begin{array}{ccc} f_1 & g_1 & h_1 \\ f_2 & g_2 & h_2 \\ f_3 & g_3 & h_3 \end{array} \right\}$$

become respectively

$$\left. \begin{array}{ccc} 0 & \frac{1}{B} - \frac{1}{C} \\ -\frac{1}{B} & 0 & \frac{1}{C} \\ -\frac{1}{C} & \frac{1}{B} & 0 \end{array} \right\},$$

and the cubic equation is resolved without extraction of roots.

It follows from my theorem that the eight intersections of three concentric surfaces of the second order can be found by the solution of one cubic and one quadratic equation; and in general, if we have ϕ, ψ, θ any three quadratic functions of x, y, z , and $\phi=0, \psi=0, \theta=0$ be the system of equations to be solved, provided that we can by linear transformations express ϕ, ψ, θ under the form of

$$U - aw$$

$$V - bw$$

$$W - cw,$$

U, V, W being homogeneous functions, and w a non-homogeneous function of three new variables, x', y', z' , we can find the eight points of intersection of the three surfaces, of which U, V, W are the characteristics, by the solution of one cubic and one quadratic. But (as I am indebted to Mr. Cayley for remarking to me) that this may be possible, implies the coincidence of the vertices of one cone of each of the systems of four cones in which the intersections of the three surfaces taken two and two are contained.

I may perhaps enter further hereafter into the discussion of this elegant little theory. At present I shall only remark, that a somewhat analogous mode of solution is applicable to two equations,

$$U = aP^2$$

$$V = bP^2,$$

in which U, V are homogeneous quadratic functions, and P some non-homogeneous function of x, y .

We have only to make the determinant of $fU + gV$ equal

to zero, and we shall obtain two systems of values of f, g , wherefrom we derive

$$l_1x + m_1y = \pm \sqrt{af_1 + bg_1} \cdot P$$

$$l_2x + m_2y = \pm \sqrt{af_2 + bg_2} \cdot P,$$

from which x and y may be determined.

26 Lincoln's-Inn-Fields,

August 28, 1850.

XLIX. *On the Meteorology of England and the South of Scotland during the Quarter ending September 30, 1850. By JAMES GLAISHER, Esq., F.R.S., Hon. Sec. of the British Meteorological Society, &c.**

THE mean daily temperature of the air was below its average value till July 13; the mean defect was $2^{\circ}2$. From July 12 to the 24th, the period was warm; the average excess of temperature was $4^{\circ}8$. From July 25 to August 3, the temperature was below the average; its mean deficiency was 1° . From August 4 to August 18, it was above the average; the mean excess was 2° ; this was followed by a long period of fine, clear, dry, but cold weather. The average deficiency of temperature between August 19 and September 17 was $3^{\circ}5$; and after September 18, the daily temperatures were slightly above their average values. Snow fell on Ben-Lomond on August 23.

The mean temperature of the air at Greenwich for the three months ending August, constituting the three summer months, was $61^{\circ}1$, being $1^{\circ}2$ above the average of the preceding seventy-nine summers.

For the month of July was $62^{\circ}2$, exceeding that of the average of seventy-nine years by $0^{\circ}9$, and of nine years by $0^{\circ}7$.

For the month of August was $60^{\circ}2$, being $0^{\circ}3$ less than the average of seventy-nine years, and $0^{\circ}9$ less than that of the preceding nine years.

For the month of September was $56^{\circ}4$, exceeding the average from seventy-nine years by $0^{\circ}1$, and less than that of the preceding nine years by $0^{\circ}7$.

The mean for the quarter was $59^{\circ}6$, exceeding that of the average of seventy-nine summer quarters by $0^{\circ}2$, and less than that of the nine preceding years by $0^{\circ}3$.

The mean temperature of evaporation at Greenwich—

For the month of July was $58^{\circ}6$; for August was $56^{\circ}6$; and for June was $52^{\circ}9$. These values are $0^{\circ}9$ greater, $0^{\circ}2$ greater, and $1^{\circ}6$ less than those of the averages of the same months in the preceding nine years.

* Communicated by the Author.

The mean temperature of the dew-point at Greenwich—

For the months of July, August and September, were $55^{\circ}\cdot 8$, $53^{\circ}\cdot 1$, and $47^{\circ}\cdot 7$ respectively. These values are $1^{\circ}\cdot 5$ greater, $2^{\circ}\cdot 0$ less, and $4^{\circ}\cdot 7$ less respectively than the averages of the same months in the preceding nine years.

The mean elastic force of vapour at Greenwich for the quarter was $0\cdot 422$ inch, being less than the average from the preceding nine years by $0\cdot 008$ inch.

The mean weight of water in a cubic foot of air for the quarter was $4\cdot 8$ grains, being of the same value as the average from the preceding nine years.

The mean degree of humidity in July was $0\cdot 88$, in August was $0\cdot 81$, and in September was $0\cdot 75$. The averages from the nine preceding years were $0\cdot 79$, $0\cdot 83$ and $0\cdot 85$ respectively.

The mean reading of the barometer at Greenwich in July was $29\cdot 789$ inches, in August was $29\cdot 787$, and in September was $29\cdot 930$. These readings are $0\cdot 010$ less, of the same value, and $0\cdot 121$ greater respectively than the averages of the same months in the preceding nine years.

The average weight of a cubic foot of air in the quarter was 527 grains; exceeding that of the average of the preceding nine years by 1 grain.

The rain fallen at Greenwich in July was $2\cdot 9$ inches, in August was $4\cdot 9$, and in September was $1\cdot 3$. The falls for the three months on an average of nine years, are $2\cdot 3$, $2\cdot 6$, and $2\cdot 3$ inches respectively.

The average daily ranges of the readings of the thermometer in air at the height of four feet above the soil, in July was $20^{\circ}\cdot 0$, in August was $18^{\circ}\cdot 6$, and in September was $17^{\circ}\cdot 1$. The averages for the three months from the preceding nine years were $19^{\circ}\cdot 4$, $17^{\circ}\cdot 6$ and $18^{\circ}\cdot 9$ respectively.

The minimum readings of the thermometer on grass, with its bulb fully exposed to the sky, in July was at or below 40° on eight nights; the lowest was 34° , and was above 40° on twenty-three nights; the highest reading was $55^{\circ}\cdot 5$. In August the readings were at and below 32° on two nights; the lowest reading was 26° ; between 32° and 40° on six nights, and above 40° on twenty-three nights; the highest reading was 58° . In September the readings were at or below 32° on nine nights; the lowest reading was 24° ; between 32° and 40° on six nights, and above 40° on fifteen nights; and the highest reading was 50° .

Thunder-storms occurred on July 2 at Liverpool; on the 4th at Uckfield and Nottingham; on the 9th at Uckfield; on the 15th at Oxford, Aylesbury, Hartwell House, Hartwell Rectory, Stone, Holkham, Norwich and Oxford; on the 16th

at Holkham, Hawarden, Liverpool, Manchester, Norwich, Nottingham and Stonyhurst; on the 17th at Greenwich, Uckfield, Aylesbury, Hartwell House, Stone, Linslade, Cardington, Leicester, Greenwich, Nottingham, and S.W. of Dunino; on the 18th at Helston, Exeter, Greenwich, St. John's Wood, Oxford, Aylesbury, Hartwell House and Rectory, Stone, Linslade, Cardington, Leicester, Durham and Nottingham; on the 23rd at Jersey and Hawarden; on the 28th at Guernsey and Helston. On August 3 at Rose Hill, Oxford; on the 5th at Holkham; on the 6th at Stone and Dunino; on the 7th at Hartwell House; on the 8th at Oxford, Hartwell House, Stone, Linslade, Cardington, Hawarden, Liverpool, York and North Shields; on the 9th at York and Hartwell Rectory; on the 12th at Greenwich, Norwich and Oxford; on the 13th and 15th at St. John's Wood; on the 19th at Liverpool; on the 20th at Holkham and Nottingham; on the 21st at Nottingham; on the 24th at Greenwich and Hartwell House; on the 27th at Guernsey; on the 29th at Guernsey and Helston; and on the 30th at Guernsey. On September 20 at Exeter; on the 23rd at Holkham and Norwich; on the 24th at Holkham; on the 26th at Stonyhurst; and on the 30th at Jersey and Trowbridge.

At Uckfield, during the third week of July, the weather was very wet, and many places in this county were visited by severe thunder-storms.

At Hartwell Rectory, on the 15th of July, at 1^h 30^m P.M., there was a storm with thunder and lightning, and rain fell to the depth of 0.51 inch. July 17, at 6^h 30^m P.M., there was another thunder-storm, but very little rain; sheet lightning occurred at intervals during the evening to the south and west. July 18, at 3^h 30^m P.M., there was a thunder-storm with heavy rain; and sheet lightning was seen all the evening, followed by continued rain, which measured in the gauge, on the following morning, 1.610 inch.

At York, on August 8, a thunder-storm occurred between six and eight in the evening. The Diocesan School and the Roman Catholic Chapel were struck by lightning and injured. Sheep were killed, and two individuals were knocked down, but no human life was lost. This was the severest storm that has occurred in York for the last twenty years. Thunder and lightning occurred again on the 9th.

At Stonyhurst, the lightning during the thunder-storm of July 16 was the most brilliant Mr. Weld remembers ever to have witnessed. It frequently resembled the explosions of fireworks; and on several occasions three or four branches darted from the same centre, while sometimes the sky seemed

traversed in every direction by streaming lightning of the most vivid description. The thunder was incessant, but very distant, and no rain fell. Mr. Weld heard of seven persons being killed, and about as many more struck, but not killed, besides several valuable cows and horses which were killed.

Thunder was heard, but lightning was not seen, on July 4 at North Shields; on the 9th at Holkham; on the 15th at Guernsey; on the 16th at St. John's Wood, Linslade, Stone and Wakefield; on the 17th at Greenwich, Durham and North Shields; on the 18th at Wakefield; on the 19th at Stone; and on the 23rd at Guernsey. On August 6 at Oxford, Aylesbury, Holkham and North Shields; on the 12th at Uckfield, Linslade, Holkham, Hawarden and Liverpool; on the 13th at Jersey; on the 19th at Norwich; on the 21st at Dunino; on the 23rd at Cardington; on the 24th at Exeter, Oxford, Hartwell Rectory and Stone; and on the 28th at Nottingham. On September 3 and 24 at Aylesbury; on the 26th at Durham and North Shields; and on the 27th at St. John's Wood.

Lightning was seen, but thunder was not heard, on July 8 at Uckfield; on the 15th at Uckfield, Hartwell Rectory, Stone and Stonyhurst; on the 16th at Leicester, Nottingham and Manchester; on the 17th at St. John's Wood, Oxford, Hartwell Rectory and Liverpool; on the 19th at Stone; and on the 29th at Manchester. On August 5 at Cardington and Stone; on the 6th at Highfield House; on the 8th at Stonyhurst; on the 9th at Cardington; on the 16th at North Shields; on the 22nd at Norwich and North Shields. On September 23 at Uckfield, Greenwich, Linslade and Cardington; on the 24th at Greenwich, Oxford and Stone; on the 29th at Hartwell Rectory; and on the 30th at Helston, Uckfield, Greenwich, St. John's Wood, Oxford, Hartwell Rectory and Linslade.

Auroræ Boreales were seen at Nottingham on July 5; on July 12 at Norwich. On August 6 at Stone; on the 21st at Stone and Dunino. On September 6 and 10 at Nottingham; on the 13th at Nottingham and Hawarden; on the 14th at Stone; and on the 28th at Hartwell House, Hartwell Rectory and Stone.

Hail fell on July 12 at Hawarden; on the 20th at Oxford and Liverpool; and at Dunino on the 21st and 22nd. On September 29 at Guernsey; and on the 30th at Jersey.

Snow fell on Ben Lomond on August 23.

Frost.—The first frost was seen on August 22 at Uckfield, when the wheat and barley sheaves were frozen into a stiff mat; and Mr. Prince saw ice as thick as a wafer upon his

cucumber frames. On September 5 there was a sharp frost at Hartwell House, and at Trowbridge on September 7 and 8.

Solar halos were seen on July 6 at Uckfield; on the 10th near Oxford and Nottingham. On August 3 at Dunino; on the 7th at Greenwich; on the 20th at Dunino; on the 28th at Uckfield; and on the 29th at Exeter and Nottingham. On September 12 at Guernsey; and on the 29th at Dunino.

Lunar halos were seen on July 22 at Stone, Nottingham and Norwich. On August 21 at Uckfield and Nottingham; on the 22nd at Uckfield, Oxford, Cardington and Nottingham; on the 23rd at Uckfield and Nottingham; on the 24th at Hawarden; on the 26th at Stonyhurst; and on the 31st at Durham. On September 18 at Jersey, Guernsey, Oxford and Hawarden; on the 21st at Oxford, Hartwell Rectory, Cardington, Stone and Durham; on the 22nd at Oxford, Hartwell Rectory, Cardington, Norwich and Stone; on the 24th at Oxford; on the 25th at Cardington; and on the 26th at Durham.

Lunar coronæ were seen at Hartwell Rectory on August 14 and 16.

Lunar rainbows were seen on August 20 at Exeter; and on August 22, the Rev. C. Lowndes, at 10^h 40^m p.m., when standing on Battersea Bridge, London, saw a perfect lunar rainbow immediately under the Great Bear. The moon was shining very bright at the time, and a shower was passing (toward the north) from west to east.

Fog.—On July 11 at Stone; on the 12th at Stone and Hartwell. On September 11 at Greenwich; on the 12th at Stone, Hartwell House and Trowbridge; on the 15th at Hartwell House and Trowbridge; on the 18th at Trowbridge; on the 19th at Hartwell House and Trowbridge; on the 24th at Stone and Hartwell House; and on the 25th at Stone, Hartwell Rectory and Greenwich.

Whirlwind.—On September 30, during a thunder-storm, a whirlwind was seen by G. A. Fryer, Esq., at Trowbridge, caused by the meeting of two currents from the north-west and east. They took a southerly direction, and coming in contact with a thatched house, carried the thatch to a distance of sixty yards, and then meeting with three elm-trees, it broke the tops off and carried them to a distance of some thirty yards: the diameters of the parts of the trees where broken off were about fifteen inches.

Wheat began to be gathered in Jersey on July 15; at Hawarden, on July 29, cutting of oats; at Guernsey and Exeter on July 30. On August 1 at Nottingham; on the 2nd at Linslade and Cardington; on the 3rd at Leicester; on the

5th at Aylesbury; on the 8th at Oxford; on the 9th at Holkham; on the 12th at Durham; on the 19th at Stonyhurst and North Shields; and on the 26th at Dunino.

Harvest finished.—On August 30 at Guernsey; on the 31st at Cardington. On September 5 at Holkham; and on September 21 at Hawarden.

Remarkable rain.—At Guernsey, on August 8, rain to the depth of 1·333 inch fell in sixteen hours; and on September 28, upwards of an inch of rain fell during twelve hours.

At Falmouth, on September 24, rain to the depth of 1·93 inch fell, of which 0·8 inch fell in little more than half an hour.

At Exeter, from August 25 to September 19, no rain fell, and the weather was close, warm and fine for several days; the sky was cloudless: the average reading of the barometer was about 30·25 inches.

The amount of rain which fell during the thunder-storm on September 20 was 1·95 inch, which is the amount by which the rain in the month exceeded the average; the former being 4·33 inches, and the latter 2·39, or rather more than one-half.

At Uckfield, on July 17, the depth of rain which fell within an hour was 1·81 inch, which is almost an unprecedented amount to have fallen in so short a time in the south of England. Much heavy rain fell during the last week of September, which was very beneficial to the autumnal crops.

At Southampton no rain fell till the 21st of September; and on September 27 it fell to the depth of 1·13 inch.

At Aylesbury, on July 15, rain to the depth of 0·75 inch fell in forty-two minutes. No rain fell from the 27th of August to the 20th of September, and much inconvenience is still felt from the short supply of water.

At Cardington the springs became nearly dry during the first week in September, and continued so till the end of the month.

At Derby, the amount of rain which has fallen in the nine months of this year is 15·6 inches; the average is 22·4 inches.

At Norwich, on July 26, rain fell to the depth of 1·18 inch.

At Holkham, on July 16, rain fell to the depth of 1·29 inch in five hours and three-quarters.

At York, on August 8, rain to the depth of 0·7 inch fell within two hours. No appreciable quantity of rain fell in York between the 28th of August and the 20th of September.

At Stonyhurst, on August 5, rain fell to the depth of 0·784 inch, and on August 7 to the depth of 0·858 inch.

At North Shields, on July 25 and 26, rain fell to the depth of 1·482 inch. The month of September was remarkably

fine and dry till the 20th; on that day there was a heavy fall of rain, amounting to 0·76 inch, in five or six hours.

Meteors.—At Uckfield, meteors were very numerous during the nights of July 12, 16, 30; August 9; September 10, 11, 12.

At Hartwell Rectory, on August 11, a large meteor was seen at 10^h 10^m P.M.

At Stone, on July 13, at 11^h 20^m P.M., a meteor passed from Arcturus to Petersen's comet.

On July 29, at 9^h 57^m P.M., a meteor crossed Corona Borealis from N. to S.

September 6, at 11^h, a meteor passed from Pisces to Fornalhaut.

September 17, at 10^h 4^m P.M., a meteor passed from α Corona Borealis to 4° above Saturn.

September 28, at 9^h 30^m, a meteor as bright as Capella shot from α Draconis to γ Ursæ Majoris.

At Stonyhurst, fine meteors were seen on August 14, 23, 26 and 29.

On July 4, at 9^h 26^m P.M., a meteor, which increased in brilliancy and size as it progressed, until from a mere point it attained a size equal to three times the apparent diameter of Jupiter, and was nearly six times as bright as that planet; its colour was pale blue; and it fell nearly perpendicularly downwards, inclining very slightly towards the E. It passed from half-way between λ and θ Antinous, fading away 2° to the E. of α Capricorni, and on the same level with that star. Its motion was slow; duration 2 seconds; at first unaccompanied by sparks; finally it suddenly separated, and almost instantaneously vanished.

On July 9, at 10^h P.M., a meteor was seen twice the size of Jupiter, and similar in colour; it fell downwards from the constellation of Coma Berenices.

On August 1, at 10^h P.M., a small meteor with a train of light fell downwards from α Aquila.

On August 3, at 10^h 55^m P.M., a meteor, equal in size to a star of the fifth magnitude, fell rapidly from α Corona Borealis to ζ Bootis; its duration was 0^s·5, and it instantly disappeared.

On August 6, at 10^h P.M., three small meteors were seen by A. S. H. Lowe, Esq. Another meteor was seen at 10^h 22^m P.M., which fell from ϵ Pegasi to β Aquarii, leaving a train of light for 20^s afterwards.

On August 8, at 10^h 20^m P.M., a meteor was seen by A. S. H. Lowe, Esq., which fell from ϵ Ursæ Majoris; at 11^h 15^m P.M. a meteor fell from α Ophiuchi.

On August 9, at 11^h 15^m P.M., two meteors were seen, one being in the zenith.

On August 12, at 10^h 32^m P.M., E. J. Lowe, Esq. saw a meteor which moved horizontally, and which increased in brilliancy from being equal to a star of the fifth magnitude to one of the second. Its colour was blue, and duration 0^s.2. Its path was from 24 Camelopardalis towards λ Draconis.

On August 12, at 10^h 32^m P.M., a meteor passed τ Cassiopeia near ϕ Ursæ Majoris, and was equal in size to a star of the third magnitude. Its colour was blue, and its duration $\frac{1}{2}$ ^s.

On the same night, at 11^h 9^m P.M., a meteor fell from between β , γ and λ Pegasi, perpendicularly down to within 20° of the horizon, when it went behind a cloud; and from 1^s to 2^s after a flash resembling lightning, and quite as vivid, proceeded from behind the cloud, followed immediately by a second flash. The meteor itself was about 12' in diameter, was globular in form, and yellow in colour. It moved very slowly. This meteor was followed by a train of light.

On August 14, at 8^h 45^m P.M., a meteor was seen four or five times larger than Jupiter. It was of a pale straw-colour, very globular in form, with a red defined disc. No train of light visible. It fell from between λ Bootis and η Ursæ Majoris perpendicularly downwards. It passed 3° or 4° N. of the large group of stars in Coma Berenices. Its duration was 2^s.

On August 14, at 9^h 48^m P.M., a small meteor moved from 24 Camelopardalis to Ursæ Majoris. Its colour was blue, and duration $\frac{1}{2}$ ^s.

On the same night, at 9^h 49^m P.M., a meteor was seen in the zenith.

On August 22, at 10^h P.M., a meteor fell from ϵ Cephei through λ Andromedæ.

On August 22, at 10^h 24^m P.M., a meteor was seen about the size of Arcturus, and of a yellow colour. It fell perpendicularly down, inclining to the N., from 5° below γ Bootis.

On August 29, at 9^h 59^m 35^s, a meteor of the size of a star of the third magnitude. It was blue in colour, and moved very rapidly. It passed from η Bootis to Arcturus. Its duration was 0^s.5.

On August 29, at 10^h 1^m P.M., a meteor of the size of a star of the second magnitude. Its colour was red. It left a train of red sparks, and moved rapidly from γ Trianguli to Saturn.

On August 29, at 10^h 4^m P.M., a meteor was seen of an orange-scarlet colour. It moved slowly from ϵ Persei to near 21 Pegasi in a horizontal direction. Its duration was 2^s. When first seen it was equal to a star of the fifth magnitude, but gradually increased in diameter as it progressed until it

became three times as large as Saturn. There was no large ball of light. It disappeared suddenly.

On the same night, at 10^h 7^m P.M., a meteor was seen, which moved rather slowly, was of a blue colour, with a slight tail; duration, 1^s; in size, superior to a star of the second magnitude.

On September 1, at 9^h 5^m P.M., a meteor was seen in the zenith.

On September 2, at 11^h 13^m P.M., a meteor was seen passing rapidly from δ Aquilæ to θ .

On the same night, at 11^h 16^m P.M., a similar one from ϵ Aquarii to β Capricorni.

On the same night, at 11^h 19^m P.M., a meteor passed from α Tauris to β Ophiuchi.

Again, on the same night, at 10^h 20^m P.M., from η Ursæ Minoris to ϵ Ursæ Majoris. Duration, 1^s; colour, yellow.

On September 12, at 12^h 6^m P.M., a meteor was seen of the size of a star of the third magnitude. Its colour was blue, and moved from below α Aquila towards the west.

On September 28, at 10^h 45^m P.M., a meteor, which moved from S.S.E. to S.W., at an elevation of 45°, and leaving a long train of light behind.

The temperature of the water of the Thames, from the observations of Lieut. Sanders, R.N., Superintendent of the Dreadnought Hospital Ship, was 64°·6 in July, 63°·2 in August, and 57°·9 in September.

The daily horizontal movement of the air at Greenwich, in July was 79 miles, in August was 119 miles, and in September was 82 miles. At Liverpool, in July was 166 miles, in August was 174 miles, and in September was 129 miles. These determinations are by the use of Whewell's anemometer at both places; and Mr. Hartnup says, that his daily determinations are made in the same manner as at Greenwich.

The series of observations of the *direction of the wind*, at 9^h A.M., taken at the various railway stations, and published in the *Daily News*, has been extended during the past quarter to Ireland. The following Tables have principally been formed from them. The results for Belgium have been formed from monthly reports furnished to the Astronomer Royal:—

July 1850.	Direction of the Wind.						General Remarks.
	On the south coast.	On the south-east coast.	On the east coast.	On the north-east coast.	On the north-west coast.	On the south-west coast.	
1	S.W.	S.W.	S.W.	W.	variable.	S.W.	In the southern counties.
2	W. & S.W.	S.W.	S.W.	W.	S.W.	S.W.	In the midland counties.
3	S.W.	S.W.	S.W.	S.W.	S.W.	S.W.	In the northern counties.
4	S.W.	S.W.	S.W.	S.	S.W.	S.W.	
5	W.	W.	W.	W.	W.	W.	
6	S.	S.W.	S.W.	S.	variable.	S.	
7	N.	N.E.	N.	N.N.W.	N.	N.	
8	N.W.	N.W.	N.W.	N.W.	N.W.	N.W.	
9	N.W.	N.W.	N.W.	N.W.	W.	N.W.	
10	N.W.	N.W.	N.W.	N.W.	W.	N.W.	
11	N.W.	N.W.	N.W.	calm.	calm.	N.W.	
12	variable.	variable.	variable.	variable.	variable.	variable.	
13	variable.	n.e.	n.e.	calm.	variable.	variable.	
14	n.e.	n.e.	n.e.	n.e.	calm.	variable.	
15	n.e.	n.e.	n.e.	n.e.	e.	variable.	
16	S.W.	n.e.	n.e.	n.e.	e.	variable.	
17	N.W.	n.e.	variable.	variable.	calm.	n.	
18	n.	n.W.	calm.	calm.	S.E.	variable.	
19	variable.	S.W.	variable.	n.e.	n.W.	n.W.	
20	n.	n.e.	n.e.	variable.	variable.	n.W.	
21	W.	W.	variable.	variable.	S.W.	S.W.	
22	e.	e.	e.	variable.	S.	S.E.	
23	e.	e.	e.	variable.	S.E.	S.E.	
24	W.	W.	S.W.	S.W.	S.	W.	
25	S.	S.W.	S.	S.E.	calm.	calm.	
26	W.	S.W.	S.W.	N.W.	N.W.	W.	
27	variable.	W.	S.W.	N.N.W.	N.	N.W.	
28	N.W.	N.W.	N.	N.	N.W.	N.	
29	N.	N.	N.	N.	N.	N.	
30	n.n.e.	n.	n.n.W.	W.	N.W.	n.e.	
31	variable.	variable.	W.	W.	variable.	variable.	

Aug. 1850.	Direction of the Wind.							General Remarks.
	On the south coast.	On the south-east coast.	On the east coast.	On the north-east coast.	On the north-west coast.	On the south-west coast.	Belgium.	
1	n.e.	n.w.	n.w.	s.w.	variable.	n.e.	n.n.e.	Air in gentle motion. Fog at Hastings.
2	s.s.w.	s.w.	s.	w.s.w.	variable.	n.w.	n.n.e.	Very calm in the Southern and Midland Counties, with rain.
3	s.w.	n.e.	n.e.	w.	s.w.	s.w.	w.s.w.	Similar in character to the preceding day. Rain to the South.
5	s.	n.w.	s.w.	w.	s.	s.	s.w.	Air in very gentle motion. To the North calm and rain.
6	variable.	n.w.	n.w.	w.	s.w.	n.w.	s.s.w.	Very variable in strength, particularly on S. coast. Rain in S.
7	w.n.w.	n.	n.w.	w.	s.	s.w.	n.w.	Calm throughout. Heavy rain at Yarmouth. [of Scotland]
8	s.w.	s.w.	s.w.	s.w.	s.w.	s.w.	s.s.w.	Gentle breeze. Rain general to S. coast, over S. Counties, and in
9	w.s.w.	w.	s.w.	w.s.w.	s.w.	w.	w.s.w.	Gentle breeze to N. Hard wind S. of Leeds. [S. of Scotland]
10	w.s.w.	w.s.w.	w.	w.	s.w.	s.w.	w.s.w.	Strong breeze at both extremities of the country. Over M. Coun-
12	variable.	n.e.	e.s.e.	variable.	variable.	e.s.e.	w.s.w.	Varying from strong breeze to calm. [ties a gentle br. with fog.
13	n.e.	w.	w.	n.w.	s.w.	n.w.	s.w.	Air in gentle motion. Frosty at Whitby.
14	n.n.e.	n.	n.	n.	n.e.	n.e.	n.n.e.	Gentle breeze and calm. A strong breeze on the South coast.
15	n.w.	n.w.	s.e.	variable.	calm.	calm.	n.n.w.	Varying from calm to strong breeze. Fog to the North.
16	n.w.	n.	n.e.	n.e.	variable.	n.	s.w.	Strong breeze to the South. Calm and fog to the North.
17	w.s.w.	calm.	calm.	w.	n.	n.w.	n.w.	Calm and gentle breeze. Fog to the North.
19	w.	s.w.	s.w.	w.	w.	w.	w.s.w.	A storm and heavy gale in Scotland. A strong breeze on the
20	w.n.w.	s.w.	w.	w.	n.w.	n.w.	w.s.w.	Strong breeze at most places. [South coast]
21	variable.	s.	s.w.	w.s.w.	variable.	variable.	w.s.w.	Calm and rain to the South and Midland Counties.
22	s.w.	s.w.	s.w.	s.s.w.	s.	w.	s.s.e.	Strong breeze to the South and Midland Counties.
23	w.s.w.	s.w.	w.	w.s.w.	calm.	w.s.w.	w.s.w.	Gentle breeze and calm. [rain to the West.
24	w.s.w.	calm.	calm.	w.	n.w.	s.	variable.	Stormy with gales at Whitby. Strong br. to the S. coast. [county.
26	w.s.w.	s.w.	w.	variable.	s.	s.w.	s.s.e.	Gentle breeze at most places. Calm and rain to the North.
27	variable.	calm.	n.w.	w.n.w.	n.w.	n.e.	w.s.w.	Strong breeze at most places. Hard wind to the North.
28	n.	n.w.	n.w.	w.n.w.	n.w.	n.	w.n.w.	Strong breeze in the Midland Counties.
29	n.w.	n.w.	n.w.	w.n.w.	n.w.	n.w.	w.n.w.	Gentle breeze in most places.
30	n.	n.	n.	n.	n.w.	n.	w.s.w.	Calm and gentle breeze.
31	n.w.	n.w.	w.	n.n.w.		n.w.	w.s.w.	

Air in gentle motion. Fog at Hastings. Very calm in the Southern and Midland Counties, with rain. Similar in character to the preceding day. Rain to the South. Air in very gentle motion. To the North calm and rain. Very variable in strength, particularly on S. coast. Rain in S. Calm throughout. Heavy rain at Yarmouth. [of Scotland.] Gentle breeze. Rain general to S. coast, over S. Counties, and in Strong breeze at both extremities of the country. Over M. Coun- Varying from strong breeze to calm. [ties a gentle br. with fog.] Air in gentle motion. Frosty at Whitby. Gentle breeze and calm. A strong breeze on the South coast. Varying from calm to strong breeze. Fog to the North. Strong breeze to the South. Calm and fog to the North. Calm and gentle breeze. Fog to the North. A storm and heavy gale in Scotland. A strong breeze on the Strong breeze at most places. [South coast.] Calm and rain to the South and Midland Counties. Calm and Strong breeze to the South and Midland Counties. Calm and Gentle breeze and calm. [rain to the West.] Calm and rain in M. Counties. Gentle breeze at both extrem. of Stormy with gales at Whitby. Strong br. to the S. coast. [country.] Gentle breeze at most places. Calm and rain to the North. Strong breeze at most places. Hard wind to the North. Strong breeze in the Midland Counties. Gentle breeze in most places. Calm and gentle breeze.

Sept. 1850.	Direction of the Wind.										General Remarks.
	On the south coast.	On the south-east coast.	On the north-east coast.	On the north-west coast.	On the south-west coast.	In the southern counties.	In the midland counties.	In the northern counties.	Ireland.	Belgium.	
2	n.w.	s.w.	w.	n.w.	variable.	w.	w.	w.	n.w.	n.w.	Gentle breeze at most places. Strong br. on N.E. coast.
3	n.n.e.	n.	e.	calm.	n.n.e.	n.e.	e.n.e.	e.n.e.	calm.	e.n.e.	Calm and gentle breeze. Rain in the North.
4	e.n.e.	n.e.	n.e.	calm.	e.n.e.	n.e.	e.n.e.	n.n.e.	s.e.	e.n.e.	Gentle breeze and calm. Fog to the North.
5	n.n.w.	n.w.	n.e.	calm.	n.e.	n.	calm.	calm.	calm.	n.	Calm general. Fog to the North.
6	n.w.	n.w.	n.w.	s.e.	e.s.e.	n.	n.e.	variable.		n.n.w.	Strong breeze to the South. Gentle breeze to the N.
7	n.n.e.	n.	n.w.	variable.	w.s.w.	n.w.	n.w.	w.n.w.	n.w.	n.w.	Strong breeze over S. and M. Counties. Gentle br. to S.
9	n.	n.	n.w.	calm.	e.s.e.	n.e.		calm.	e.s.e.	variable.	Nearly calm. Much fog to the North.
10	n.n.e.	n.e.	calm.	calm.	e.s.e.	e.	s.e.	variable.	e.s.e.	n.n.e.	Calm and gentle breeze. Fog at Lanark.
11	e.n.e.	s.e.	calm.	e.s.e.	e.s.e.	e.	e.s.e.	variable.	s.s.e.	e.n.e.	Calm and gentle breeze.
12	n.e.	e.	e.	calm.	e.s.e.	e.	e.s.e.	s.e.	s.e.	e.s.e.	Air in gentle motion. Calm at many places.
13	e.	e.	e.	s.e.	e.s.e.	e.	s.e.	e.	s.s.w.	e.s.e.	Gentle breeze. Fog to the North.
14	e.	e.	e.	s.e.	e.	e.	n.e.	s.e.	s.e.	n.e.	Strong breeze to the S. coast. Calm and fog to the N.
16	n.e.	e.	e.	calm.	e.n.e.	e.	e.	calm.	calm.	e.s.e.	Calm to the N. Strong breeze in the M. Counties.
17	e.n.e.	n.e.	calm.	calm.	e.n.e.	e.n.e.	e.s.e.	calm.	calm.	n.e.	Strong breeze to the South. Calm and fog to the N.
18	e.n.e.	e.	n.e.	s.	e.s.e.	variable.	calm.	variable.	s.s.e.	w.n.w.	Calm and gentle breeze. Fog at Dundee.
19	e.n.e.	s.e.	s.e.	w.	s.s.e.	e.	e.	variable.	calm.	w.s.w.	Calm and fog at most places.
20	s.e.	s.e.	s.e.	s.s.e.	n.	e.	s.e.	variable.	s.	s.s.e.	Gentle breeze and rain at most places.
21	w.s.w.	s.w.	s.w.	s.s.e.	w.s.w.	s.w.	s.s.w.	s.s.e.	w.s.w.	w.s.w.	Hard wind S. and M. Counties. Rain and strong br. to N.
23	w.n.w.	w.	n.w.	calm.	w.n.w.	calm.	variable.	calm.	w.n.w.	s.s.w.	Calm nearly everywhere.
24	n.w.	calm.	s.e.	calm.	n.n.e.	n.n.w.	n.n.w.	w.	calm.	s.s.w.	Calm and fog general.
25	calm.	s.w.	calm.	calm.	s.s.w.	calm.	calm.	calm.	s.w.	w.s.w.	Calm and fog at nearly every station.
26	s.w.	s.w.	s.	s.s.w.	w.s.w.	s.	s.	variable.	s.w.	s.s.w.	Calm and rain general.
27	variable.	s.w.	s.w.	s.s.e.	w.s.w.	w.s.w.	w.s.w.	s.w.	s.w.	s.s.w.	Air in gentle motion. Rain at many places.
28	w.	w.	n.w.	w.	w.s.w.	n.w.	w.s.w.	s.w.	n.w.	w.s.w.	Strong breeze at most places. Gale and hard wind to N.
30	w.	w.	s.w.	n.w.	w.	variable.	n.e.	n.e.	n.e.	s.w.	Strong breeze and hard wind at many stations.

The observations have been taken to mean values, and the hygrometrical results have been deduced from Glaisher's Tables.

Names of the places.	Mean pressure of dry air reduced to the level of the sea.	Mean temperature of the air.	Highest reading of the thermometer.	Lowest reading of the thermometer.	Mean daily range of temperature.	Mean monthly range.	Range of temperature in the quarter.	Mean temperature of the dew-point.	Wind.		Mean amount of cloud.	Number of days on which it fell.	Rain.	Mean weight of vapour poured in a cubic foot of air.	Mean additional weight of vapour required to saturate a cubic foot of air.	Mean degree of humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Height of cistern of the barometer above the level of the sea.
									Mean estimated strength.	General direction.									
Jersey	29.652	60.0	81.0	48.0	14.4	30.7	33.0	54.5	1.7	s.w. & n.w.	4.8	26	64	4.9	0.5	0.822	in.	529	84
Guernsey	29.599	60.1	75.5	51.5	8.9	19.2	24.0	56.2	1.7	w. & c.	4.9	36	68	5.2	0.6	0.894	6.4	529	84
Helston	29.614	59.7	89.0	49.0	15.3	35.3	40.0	54.6	1.4	s.w. & n.w.	5.8	37	7.2	4.9	0.9	0.839	6.0	528	106
Falmouth	29.604	59.1	78.0	49.0	18.5	33.3	38.0	53.7	1.4	n.	6.6	33	10.8			0.834	5.4	530	120
Truro	29.604	58.5	77.0	47.0	14.8	34.3	38.0	53.7	1.0	n.	5.9	41	4.8	4.8	1.0	0.834	5.4	530	55
Torquay	29.604	59.3	77.0	47.0	12.2	26.3	37.0	52.8	2.3	n.	5.9	20	8.0	4.7	1.3	0.780	5.7	530	160
Exeter	29.600	59.1	81.0	46.7	15.8	35.7	40.9	53.1	1.7	w.	3.7	36	8.0	4.7	1.2	0.804	5.9	528	140
Uckfield	29.598	59.5	89.0	31.0	22.3	35.3	45.0	51.8	0.3	w.	6.1	33	9.1	4.5	1.5	0.747	5.5	528	180
Southampton	29.589	59.7	81.0	36.0	17.0	38.9	45.0	51.8	0.3	w.	6.1	33	9.1	4.5	1.5	0.747	5.5	528	180
Royal Observatory, Greenwich.	29.609	59.6	87.0	39.0	18.6	38.0	48.0	52.2	0.3	n.e. & s.w.	6.8	33	5.7	5.0	1.2	0.788	5.6	527	159
Maidenstone Hill, Greenwich.	29.608	58.5	84.3	39.6	14.6	35.0	44.7	54.8	0.3	n.e. & s.w.	6.8	33	5.7	5.0	1.2	0.788	5.6	527	159
St. John's Wood.	29.649	57.5	83.0	33.0	16.0	37.0	45.0	49.6	0.3	s.w.	6.5	37	5.9	4.5	1.8	0.779	5.1	530	107
Chiswell Street, London	29.649	61.2	80.0	46.0	21.4	27.3	34.0	51.5	0.7	s.	6.5	37	5.9	4.5	1.8	0.779	5.1	530	107
Aylesbury	29.650	59.2	86.0	32.0	21.2	46.4	54.0	51.1	0.7	s.	6.5	37	5.9	4.5	1.8	0.779	5.1	530	107
Stone Observatory	29.600	57.8	85.5	33.0	19.9	42.0	52.5	50.7	0.9	s.w.	5.6	46	5.8	4.4	1.4	0.765	5.4	523	284
Hartwell (near Aylesbury)	29.580	58.6	88.0	32.0	23.4	46.0	56.0	52.6	0.9	n.w. & s.w.	5.8	32	6.1	4.6	1.0	0.817	5.3	525	310
Hartwell Rectory	29.602	57.1	81.5	33.5	17.2	30.0	48.0	49.6	0.6	n.w. & s.w.	5.8	32	6.1	4.6	1.0	0.817	5.3	525	250
Linslade (Bucks)	29.602	57.3	85.5	35.0	19.3	40.5	50.5	50.5	1.9	n.e. & s.w.	7.2	38	8.0	4.5	1.0	0.813	5.5	528	313
Ratcliffe Observatory, Oxford.	29.608	57.2	79.6	35.1	15.8	35.6	41.5	51.5	1.9	n.e. & s.w.	7.2	38	8.0	4.5	1.0	0.813	5.5	528	210
Rose Hill (near Oxford).	29.604	58.0	84.5	35.5	16.6	40.9	48.0	52.4	0.9	Variable.	7.1	38	7.8	4.6	1.1	0.806	5.7	529	270
Cardington	29.535	57.8	83.5	35.0	19.6	40.9	48.0	52.4	0.9	Variable.	7.1	38	7.8	4.6	1.1	0.806	5.7	529	100
Norwich	29.535	57.8	81.0	41.0	16.8	31.9	40.0	52.8	0.9	s.w. & n.w.	6.9	43	10.2	4.7	0.9	0.838	5.7	530	23
Leicester Museum	29.538	59.8	79.0	38.0	14.4	32.6	41.0	50.0	1.3	w.	7.5	31	6.1	4.3	1.1	0.791	5.2	527	175
Holkham	29.610	57.1	89.8	37.5	18.6	36.6	43.3	51.3	1.1	n.w.	6.4	47	9.2	4.4	1.0	0.821	5.4	531	39
Highfield House, Notts.	29.557	57.3	87.3	33.0	21.6	45.2	54.3	53.8	0.4	Variable.	6.5	47	7.1	4.7	0.7	0.865	5.8	529	103
Derby	29.586	56.5	82.0	32.0	18.0	45.2	54.3	53.8	0.4	Variable.	6.5	47	7.1	4.7	0.7	0.865	5.8	529	103
Haverdon	29.586	56.5	82.0	32.0	18.0	45.2	54.3	53.8	0.4	Variable.	6.5	47	7.1	4.7	0.7	0.865	5.8	529	103
Liverpool	29.592	58.5	81.5	39.5	12.7	28.0	42.0	48.8	1.7	n.w.	7.0	44	7.7	4.1	1.3	0.763	5.0	528	260
Wakefield.	29.585	53.6	83.0	46.3	10.2	24.0	34.6	53.0	1.3	n.w.	7.6	43	7.8	4.7	0.9	0.937	5.7	530	37
Stonyhurst	29.609	54.9	77.6	35.2	21.4	44.2	52.0	52.2	2.0	w.	6.7	48	6.7	4.7	0.9	0.937	5.7	530	115
York	29.612	53.7	78.0	35.0	14.5	36.0	43.0	50.9	1.3	e. to w.	6.8	49	12.3	4.3	0.8	0.818	5.2	527	281
Whitby	29.597	56.4	81.0	39.5	12.8	31.2	44.5	53.6	0.8	Variable.	6.3	43	12.5	4.6	0.9	0.833	5.3	530	50
Durham	29.596	55.9	76.8	34.8	14.8	34.3	42.0	49.4	0.8	s. & w.	6.3	44	6.3	4.2	0.8	0.880	5.0	532	80
Newcastle.	29.555	55.5	76.0	37.0	14.8	33.0	39.0	51.6	0.8	s. & e.	3.4	50	6.6	4.5	0.7	0.828	5.1	528	340
North Shields	29.668	53.5	70.4	36.8	11.1	30.0	33.6	50.8	2.8	n.e. & s.w.	3.4	50	6.6	4.4	0.4	0.927	5.3	535	124
Glasgow	29.555	55.8	79.9	39.0	14.7	33.2	40.9	49.8	1.9	Variable.	3.8	31	6.0	4.2	1.0	0.810	5.2	530	121
Dunino	29.555	56.3	81.0	37.0	20.0	39.0	44.0	49.1	1.9	Variable.	3.8	31	6.2	4.4	1.2	0.792	5.0	530	250
Number of columns	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19

The mean monthly values of the several subjects of investigation are published in the Quarterly Report of the Registrar-General. Their quarterly values are shown in the preceding table.

The mean of the numbers in the first column is 29·605 inches, and it represents that portion of the reading of the barometer due to the pressure of the air; the remaining portion, or that due to the pressure of water, is 0·397 inch; the sum of those two numbers is 30·002 inches, and it represents the mean reading of the barometer at the mean level of the sea for the quarter ending September 30, 1850.

The mean of the numbers in the second column for Guernsey and Jersey is $60^{\circ}1$; for those places in the counties of Cornwall and Devonshire, is $59^{\circ}2$; south of latitude of 52° is $58^{\circ}4$; between latitudes 52° and 53° is $57^{\circ}3$; between the latitudes of 53° and 54° is $55^{\circ}7$; at Liverpool and Whitehaven is $57^{\circ}3$; for Durham, Newcastle and North Shields, is $54^{\circ}6$; and for Glasgow and Dunino is $56^{\circ}1$.

The highest reading of the thermometer in air was about 88° , and the lowest was 32° . The extreme range of temperature during the quarter in England was therefore about 66° . The least daily ranges of temperature took place at Guernsey and Liverpool, and the greatest occurred at Uckfield and in the Vale of Aylesbury.

Rain fell on the least number of days at Jersey and Torquay, and on the greatest number of days at Wakefield, Stonyhurst and North Shields. The places where the least falls took place are London and Truro; and the mean amount at these places was 5·3 inches. The largest falls occurred at Whitehaven and Falmouth, and their average was 12·4 inches.

L. *On a remarkable property of Steam connected with the Theory of the Steam-Engine.* By WILLIAM THOMSON, Esq., F.R.S.E., Professor of Natural Philosophy in the University of Glasgow. Communicated by J. P. JOULE, Esq., F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I AM permitted by my friend Professor Thomson to communicate the following letter to the Philosophical Magazine, containing an explanation of the true cause of the non-scalding property of steam issuing from a high-pressure boiler. The proposition announced by Mr. Rankine is certainly one of very great importance; as it would appear from it that when saturated steam is allowed to expand so as to evolve

work, a part of it is condensed, and that this condensation affords heat for the expansion of the remainder of the steam. This fact, which is analogous to that of the production of a cloud when air saturated with vapour is rarefied in the receiver of an air-pump, explains the approach of the æconomical duty of the steam-engine to that of the air-engine, on which I propose to make a few observations shortly.

I have the honour to remain, Gentlemen,

Yours very respectfully,

JAMES P. JOULE.

MY DEAR SIR,

Paris, October 15, 1850.

IN Mr. Rankine's paper on the Mechanical Action of Heat*, the following very remarkable result is announced:—"If vapour at saturation is allowed to expand, and at the same time is maintained at the temperature of saturation, the heat which disappears in producing the expansion is greater than that set free by the fall of temperature, and the deficiency of heat must be supplied from without, *otherwise a portion of the vapour will be liquefied in order to supply the heat necessary for the expansion of the rest.*" This conclusion can, I think, be reconciled with known facts only by means of your discovery, that heat is evolved by the friction of fluids in motion. For it is well known that the hand may be held with impunity in a current of steam issuing from the safety-valve of a high-pressure boiler; and again, it is known that "Watt's law" does not rigorously express the actual decrease in the latent heat of saturated steam with an elevation of temperature; but, on the contrary, Regnault shows that the "total heat" of saturated steam increases slowly with the temperature, at an approximately uniform rate. These two facts are consistent and connected with one another; for, according to the latter, steam issuing from a high-pressure boiler ought, in the immediate neighbourhood and on the outside of the orifice, where, of course, its pressure scarcely exceeds that of the atmosphere, to be at a temperature sensibly above 212° , and consequently supersaturated, and quite dry; and it is well known that the hand experiences no pain from being exposed to a hot current of a dry gas, even if the temperature considerably exceeds 212° . But, according to Mr. Rankine's proposition, steam allowed to expand from saturation will, *if no heat be supplied to it*, remain saturated, except a small portion which becomes liquefied. Either then Mr. Rankine's conclusion is opposed to the facts,

* Transactions of the Royal Society of Edinburgh, vol. xx. part 1. (Read Feb. 4, 1850.)

or some heat must be acquired by the steam as it issues from the boiler. The pretended explanation of a corresponding circumstance connected with the rushing of air from one vessel to another in Gay-Lussac's experiment, on which you have commented, is certainly not applicable in this case, since instead of receiving heat from without, the steam must lose a little in passing through the stop-cock or steam-pipe by external radiation and convection. There is no possible way in which the heat can be acquired except by the friction of the steam as it rushes through the orifice. Hence I think I am justified in saying that your discovery alone can reconcile Mr. Rankine's discovery with known facts.

In connexion with this subject it is to be remarked, that if your fundamental principle regarding the convertibility of heat and mechanical effect, adopted also by Mr. Rankine, be true, a quantity of water raised from the freezing-point to any higher temperature, converted into saturated vapour at that temperature, and then allowed to expand through a small orifice wasting all its "work" in friction, will, in its expanded state, possess the "total heat" which has been given to it; but, on the contrary, if it be allowed to expand, pushing out a piston against a resisting force, it will in the expanded state possess less than that total heat by the amount corresponding to the mechanical effect developed. If the proposition quoted above of Mr. Rankine's be true, this amount must exceed the amount of deviation from Watt's law measured by Regnault; and must consequently bear a very considerable ratio to the total heat, instead of being, as I believe all experimenters except yourself have hitherto considered it to be, quite inappreciable.

In the paragraph following that from which I have quoted, Mr. Rankine remarks,—“There is as yet no experimental proof” of the preceding proposition. “It is true that in the working of non-condensing engines it has been found that the steam which escapes is always at the temperature of saturation corresponding to its pressure, and carries along with it a portion of water in the liquid state; but it is impossible to distinguish between the water which has been liquefied by the expansion of the steam, and that which has been carried over mechanically from the boiler.” The circumstances of the passage of steam through the various parts of a non-condensing engine, are certainly very complicated. Even if there were no water “carried over mechanically from the boiler,” we could not conclude the truth of Mr. Rankine's proposition from the fact of the steam issuing moist and at 212° from the waste steam-pipe, since this might be accounted for by the external

loss of heat from the cylinder, steam-pipes, &c.; nor could we conclude that Mr. Rankine's proposition is false, if the steam were observed in any case to issue dry from the steam-pipe, and at a temperature above 212° , unless the expansive principle were known to be pushed to the utmost in the actual working of the engine. It is however certain that if Mr. Rankine's proposition be true, steam, after having passed through a high-pressure engine in which the expansive principle is pushed to the utmost, whether there be any "priming" or not, and whether there be any heat lost externally from the different parts of the engine or not, will issue at the temperature of 212° , and moist (and consequently scalding to the hand), from the waste steam-pipe; and, Regnault's modification of Watt's law being considered as established, it is certain that steam issuing immediately from a high-pressure boiler into the open air will be above 212° , and dry.

The demonstration which Mr. Rankine gives of his proposition is partially founded on certain hypotheses regarding the specific heats of gases and vapours. But, besides this proposition, he derives another conclusion from the same investigation which is experimentally verified by Regnault's modification of Watt's law: and hence, as it is easy to show, if we are contented to take Regnault's result as an experimental fact, and if we adopt your mechanical equivalent for a thermal unit (or Rankine's value, which is about $\frac{7}{8}$ ths of yours), we may demonstrate Mr. Rankine's remarkable theorem without any other hypothesis than the convertibility of heat and mechanical effect.

In a paper by Clausius*, published in Poggendorff's *Annalen* for last April and May, a similar conclusion to that which I have quoted of Mr. Rankine's (whose paper was read before the Royal Society of Edinburgh on the 4th of February), is announced. I have not yet been able to make myself fully acquainted with this paper; but, from the principles and methods of reasoning explained at the commencement, which differ from those of Carnot only in the adoption of your axiom instead of Carnot's, I have no doubt but that the demonstration of the proposition in question is the same in substance as Mr. Rankine's modified in the manner I have suggested.

I remain, dear Sir, yours most truly,

J. P. Joule, Esq.

WILLIAM THOMSON.

[* A translation of this paper will appear in an early Number of this Journal.—ED.]

LI. *On the Conductibility of the Earth for Electricity.*

By JAMES NAPIER, Esq.*

AT the recent meeting of the British Association in Edinburgh, Professor Matteucci communicated a paper upon the conductibility of the earth, in which are given many interesting experiments; but it is there stated that hitherto "nothing had been known of the laws and theory of this singular phenomenon." I thought that the theory of the conductibility of the earth for electricity had been long known to men of science in this country, and the cause to be, as Mr. R. Hunt stated at the conclusion of the paper, depending upon the water contained in the superficial stratum of the earth. In corroboration of Mr. Hunt's remarks, I send you the results of a few experiments made in 1843 upon this subject, which were not published at the time, from the belief that the conclusions they led to were previously well known.

My object in these experiments was to apply the electricity obtained from the earth to the deposition of metals; the first experiments were, placing pieces of zinc and copper in the earth so as to constitute a battery; but I was soon struck with the fact that it did not appear of any consequence, as regards the quantity and power of the electric current, whether these metals were placed only a few inches apart or a number of feet, or yards, or acres, or whether there stood between them trees, houses, or streets; indeed many of the experiments were made by having one plate of metal imbedded in the garden in front and another in the garden behind my house, which had a sun's flat; and there was no apparent difference in the results obtained under these conditions to those obtained when both metals were imbedded a few inches apart in one garden. The question which suggested itself from these results was, whether this conducting power was dependent upon the nature of the materials composing the earth? To ascertain this, a plate of zinc and copper were laid upon a large table a few inches apart, and connected by a wire. When the table was wet there was a current passing, but not when dry; when these plates were imbedded in sawdust and in sand, a current was always obtained when these materials were wet, but ceased when they were dry. A large vessel was then filled with earth from the garden, and a zinc and copper plate imbedded in it, and the whole placed in such a condition as to allow the earth to dry

* Communicated by the Author.

slowly. It was interesting to remark during this experiment the gradual decline of the galvanometer needle, until the earth was perfectly dry, when it ceased to indicate any passing current. The earth was now made wet around, and in contact with the plates, leaving a portion of the earth between them dry. Under these conditions there was no current; but whenever the earth was all moistened between the plates, the current of electricity became as strong as at first. A common Wollaston's battery, charged with acid, having the two terminals or poles connected by the earth in the vessel, gave similar results; when the earth was dry there was no conduction; when wet, the current passed through easily. All these led to one conclusion, that the conducting medium was the water, which, if I mistake not, had been pointed out some time previous to my experiments, both by Mr. Hunt and Mr. Fox. And they are also the conclusions which Professor Matteucci has more recently come to.

Glasgow, 17th October, 1850.

LII. *Notices respecting New Books.*

On the Strength of Materials; containing various original and useful Formulæ, specially adapted to Tubular Bridges, Wrought Iron and Cast Iron Beams, &c. By THOMAS TATE, *Author of the Principles of the Differential Calculus, Factorial Analysis, &c.* London: Longmans, 1850.

THIS work contains in ninety-six octavo pages the usual formulæ, besides those stated to be new, for computing the relative strength of beams of various forms. The notation appears to be well chosen, and the formulæ are neat, perspicuous, and convenient for use. Special attention has been given to the subject of hollow beams and tubular bridges; and the advantages of the cellular structure, angle irons, and other contrivances adopted in the great engineering works of the kind recently constructed, are duly set forth and explained. The book, we think, cannot fail to be appreciated by the practical engineer; though a few tables showing the strength of the materials principally used in construction, and a somewhat greater variety of numerical examples, might possibly have rendered it more generally useful.

LIII. *Intelligence and Miscellaneous Articles.*

ON THE MINERALS OF THE AURIFEROUS DISTRICTS OF WICKLOW. BY WILLIAM MALLET, ESQ.

THE circumstances attending the original discovery of native gold in the beds of some of the streams of the County of Wicklow, have been already often detailed, and will therefore need but a brief repetition. The source of the auriferous streams is the mountain Croghan Kinshela, whose summit forms a portion of the boundary between the counties of Wicklow and Wexford. The stream from which most of the gold has been obtained rises on the north-east side of this mountain, and flowing down one of the glens with which that part of the country is intersected in almost every direction, joins the Aughrim river, a little above the confluence of the latter stream with the Avonmore. It receives several smaller streams at different parts of its course, in all of which *some* gold appears to have been found, though in general in such small quantity as not to repay the cost of its extraction.

Although this part of the country, since it has been known to be auriferous, has been an object of some attraction to mineralogists, but little attention seems to have been directed to the other minerals which are to be found accompanying the gold in the alluvial deposits. These, however, are interesting, not only from their number and variety, but also from the occurrence amongst them of some of the rarer species, which do not appear to have been noticed in any other locality in Ireland. The following minerals were obtained from a considerable mass of sand and gravel taken from various parts of the bed of the principal stream :—

Gold.	Galena.
Platina.	Sulphuret of molybdenum.
Tinstone.	Sapphire.
Magnetic oxide of iron.	Topaz.
Micaceous iron.	Zircon.
Red iron ochre.	Garnet (two varieties).
Hydrous peroxide of iron.	Quartz.
Common clay ironstone.	Prase.
Iron pyrites.	Augite.
Titaniferous iron.	Chlorite.
Wolfram.	Felspar.
Oxide of manganese.	Mica.
Copper pyrites.	

The author has since observed, in addition to those here mentioned, arsenical iron, in small fragments, and also spinelle. The latter occurs in very small grains along with the second variety of garnet, from which it is readily distinguished by its peculiar purplish-red colour.

Gold.—This mineral occurs here in probably its most beautiful form. It possesses the true golden yellow colour and metallic lustre

which characterize the metal, and, owing to the attrition to which it has been subjected, generally presents a beautifully brilliant surface. It occurs in grains of all sizes, from the smallest spangle up to a mass weighing twenty-two ounces, the largest hitherto found. The specific gravity of some small grains Mr. Mallet found to be 16·342. The analysis of these grains gave—

Gold	92·32
Silver.....	6·17
Iron	·78
	99·27

This is equivalent (neglecting the iron) to $8\frac{1}{4}$ atoms of gold and 1 of silver.

Platina.—Mixed with the gold are some very small flattened grains of a white colour and metallic lustre, which, as far as their minute size permitted an examination, appear to present all the characters of platina. They are infusible before the blowpipe, and insoluble in nitric acid, but dissolve in aqua-regia. Their occurrence intermixed with the gold when all other minerals have been washed off, is a proof of their high specific gravity*.

Tinstone.—The occurrence of this mineral in the sand is mentioned by Weaver in his reports on the gold-stream-works, but he does not seem to have been at all aware of the large quantities in which it exists. From the comparatively small portion† of sand which the author had an opportunity of examining, he obtained about $3\frac{1}{2}$ pounds of stream tin; a portion of which being reduced, yielded an ingot, which, when refined by a second fusion, is hardly inferior to the finest grain tin‡. Should this mineral be found in the mass of the sand in a quantity at all approaching that in which it existed in the specimen from which this was obtained, it would probably richly repay the labour and expense of its collection and smelting. From the small quantity in which other minerals of high specific gravity exist in the sand, and the constant supply of water, very little difficulty would be experienced in separating it from the rest of the sand; and the almost total absence of arsenic and lead would render it extremely easy to obtain from it metallic tin of the very first quality. The mineral itself occurs in grains varying in size from fine sand up to pebbles of half an inch in diameter, and for the most part of a dark brown colour, with some fragments of various tints of yellow and red; some presenting the peculiar appearance to which the name “wood tin” has been given. All these varieties are slightly translucent, some of them highly so. Many of them present distinct traces of the obtuse octohedron, the same

* It is to be wished that the existence of platina had been more fully ascertained.—Ed. Phil. Mag.

† The exact weight of the specimen examined the author does not know, but thinks it certainly did not exceed 150 lbs.

‡ The specimen smelted in this experiment yielded about 61 per cent. of tin; but more would be obtained on the great scale, as in this case no pains were taken to extract the tin remaining in the scorix.

with a short four-sided prism interposed between the two pyramids, and the latter of these with various truncations of its angles and edges. The specific gravity of some picked crystals was 6·753. A careful analysis of this tinstone gave as its constituents—

Peroxide of tin	95·26
Peroxide of iron	2·41
Silica	·84
	98·51

The greater number of the minerals here enumerated are mentioned by Mr. Weaver in his reports to Government on the district, and which are to be found in the Transactions of the Royal Dublin Society ; but some of them, the author believes, have not been noticed before, at least he has seen no published account of the occurrence in this locality of platina, titanite iron, sulphuret of molybdenum, topaz, zircon, the small manganesian garnets, or augite. Hence it seemed interesting, while noticing these, to collect into a uniform and as far as possible complete list, all the scattered notices of the mineral wealth of this particular district, which are to be found in Mr. Weaver's papers already referred to, and elsewhere.

The principal point, however, with respect to the examination of these minerals, which appears to merit further and more particular attention, is the fact of the existence of tinstone in such considerable quantity in these auriferous streams ;—a fact which would seem to indicate the probable existence somewhere in the surrounding district, of masses of the ore of this valuable metal of great extent, and possibly forming the continuation, on this side of the Channel, of those vast deposits which have contributed to furnish occupation and support to the inhabitants of Cornwall for more than two thousand years.—*Transactions of the Geological Society of Dublin.*

ON PYROGLYCERIN. BY M. SOBRERO.

M. Sobrero has given the above name to a compound which he obtained by treating glycerin with a mixture of nitric and sulphuric acids, in the same proportions as for preparing gun-cotton. This product is liquid, and explodes very violently ; its taste is very distinctly bitter, and is a very active poison ; two or three centigrammes immediately kill a dog. It is a powerful oxidizer ; mixed with nitric acid, it forms a kind of aqua regia. It has not been analysed, but is suspected to contain nitric acid.—*Journ. de Pharm.*, Avril 1850.

PREPARATION OF SULPHUROUS ACID. BY M. BOUTIGNY.

The author having occasion to prepare a large quantity of sulphurous acid, advantageously employed a cast-iron apparatus for this purpose in decomposing sulphuric acid by charcoal. In this operation the iron was not at all acted upon. Although many manufac-

turers have long used similar apparatus for obtaining this acid, and particularly the vessels employed in the preparation of ammonia, M. Boutigny has thought it proper to mention the above result, which may be useful to certain persons, as even the most recent chemical works recommend the preparation of sulphurous acid in glass or earthen vessels, which are always very expensive, and dangerous on account of their great fragility.—*Ibid.*

ON THE ALTERATION WHICH WELL-WATER UNDERGOES.

BY M. C. BLONDEAU.

The analysis which the author has performed on the water of a great number of the wells of Rhodéz, has led him to adopt the following conclusions:—

1. Well-water may be altered by two causes; by the presence of mineral salts held in solution, and by that of animal matters.

2. The mineral substances which occur in solution are silica, alumina, carbonates and phosphates of lime and magnesia, potash-alum, chlorides of calcium, magnesium and sodium, with nitrates of the same bases. These different substances are not hurtful to the animal œconomy when they exist only in small quantity. Well-water, [of which a litre?] contains only 4 to 5 centigrammes of these substances in solution, may be employed for all domestic uses, provided it does not contain too large a proportion of animal matter.

3. Water, of which a litre contains one gramme of the above-mentioned substances, may still be good for drinking; but it is not fit for cooking vegetables or washing linen when it contains 0.1 grm. of lime or magnesia.

4. Water, of which a litre contains 0.1 grm. of lime or of magnesia, and 0.1 grm. of organic matter, is improper for any domestic use.

5. It is of the utmost importance to state the existence and determine the quantity of animal matter held in solution in waters; for if they exceed the limits above-stated, they act disastrously on the œconomy, and may occasion dysentery, and various maladies which appear to be contagious because the whole population acquire the seeds at the same sources.

6. The presence of magnesia in drinkable waters does not produce so hurtful an action as supposed by some persons. The well-water of Rhodéz contains on an average five times as much magnesia as the waters of the valley of the Iser, analysed by M. Granger; and yet endemic diseases, as goître and cretinism, are entirely unknown in the chief town of Aveyron.

7. The water of certain wells possesses a very disagreeable earthy taste; this taste is derived from alumina held in solution by carbonic acid. It is observed that those well-waters which contain most of this base have the strongest earthy flavour.

8. It results from these experiments, that a classification of drinkable waters, based on the relations which exist between the sulphates and the chlorides, must be a defective one; for this relation varies

with respect to the same kind of water, within limits of considerable extent; and it is never certain that the water operated on has not met in its course, either above or below the soil, with substances which have altered and changed the proportions in which these salts enter into its composition.—*L'Institut*, No. 851.

ON EMERY, AND THE MINERALS ASSOCIATED WITH IT.

M. J. Laurence Smith has read a memoir before the French Academy on the Emery of Asia Minor, its geological and commercial relations, and on the minerals associated with it.

Previously to 1846, emery had not been stated to exist either in Asia Minor or the surrounding islands, except perhaps in the Isle of Samos, where it was mentioned as occurring by Tournefort in his Travels in the seventeenth century. Numerous and important deposits are now known in these countries: and Mr. Smith, by direction of the Turkish government, has explored them. He has chiefly examined this mineral *in situ* near the ruins of the ancient city of Magnesia on the Meander, and at first at the village of Gumuch, at the foot of a mountain of the same name.

With respect to this deposit, it is to be observed that all the surrounding rocks and country appear to belong to the ancient series. The limestone is entirely free from fossils, and possesses metamorphic characters; it lies upon schists, of which the micaceous appears to be the most abundant, and this further to the north occurs in contact with gneiss. In some situations there are found in the limestone, deposits of siliceous schists containing small fragments of titaniferous iron. The limestone is of a light blue colour, passing into granular marble. Towards the south, the rocks by their decomposition produce lofty precipices, which add much to the picturesque appearance of the country. The emery is found in different places in the mountain of Gumuch; it is, however, most abundant upon a part of the summit, at about a league and a half from the village of Gumuch, and 2000 feet above the level of the valley. This summit commands the magnificent plain of the Meander, the tortuous windings of which look as if they had been drawn upon a map. The emery occurs scattered on the soil in angular fragments of a deep colour. Enormous blocks weighing many tons project from the surface of the soil; and by digging, blocks of emery of different sizes are met with. On going a little deeper the rock which contains it is arrived at.

On breaking the marble which in this place projects from the soil, fragments of emery are always found.

Sometimes this mineral forms a mass of several feet long and wide, and they are now working a mass which is from thirty to sixty feet square; all the rocks which they obtain are emery. The spaces which occur, which are merely fissures formed by the contraction of this substance at its formation, are filled with earth containing oxide of iron. In some places the masses are consolidated by carbonate of lime introduced by infiltration, and which must not be confounded with the marble which is the original gangue of the emery. In

the infiltrated carbonate of lime sometimes round fragments, and at other times angular fragments of emery occur; it is never met with in veins, or with any appearance of stratification.

After Gumuch-Dagh, Kulah is the most important locality of Asia Minor. Kulah is a city about thirty leagues distant from Gumuch, seven leagues from the ancient city of Philadelphia, and not far from the river Hermes. Emery exists also in many other localities. After having studied this mineral, and the rocks which accompany its various deposits, Mr. Smith is of opinion that the emery was formed and consolidated in the calcareous rock in which it is found; and that it has not, like granite or gneiss, been detached from more ancient rocks, and deposited in the calcareous rock when the formation of the latter occurred.—*L Institut*, Juillet 17, 1850.

ON THE IDENTITY OF THE EQUISETIC, ACONITIC AND CITRIDIC ACIDS, AND ON SOME ACONITATES.

M. Baup observes that doubts existed as to the identity of the pyrogenous citridic and maleic acids; in order to decide this question, he extracted the acid from the *Equisetum fluviatile*, and from the *Aconitum napellus*, and compared them with the pyrogenous citric acid, called citridic acid, and with maleic acid.

From the comparative examination which the author made of their properties and several combinations, he has concluded with certainty that the aconitic, equisetic and citridic acids, are one and the same acid, and that they ought to be called exclusively aconitic acid, whatever may be the source from which they are procured. Maleic acid, although isomeric with it, is not however identical, and ought to retain its name.

In examining several aconitates, M. Baup has met with a fact which merits the attention of chemists, as being the first example of a compound of three atoms of an organic acid with one atom of base. The ter-aconitate of potash and also of ammonia have but very few representatives, even in inorganic chemistry: as an example, the teriodate of potash of M. Serullas may be mentioned.

During these researches on the *Equisetum fluviatile*, M. Baup discovered in it a peculiar yellow crystalline matter, which imparted to aluminated cotton a yellow tint not inferior to weld. He has given it the name of *flavequisitin*.—*Comptes Rendus*, Sept. 9, 1850.

ON THE PRODUCTION OF SUCCINIC ACID BY FERMENTATION.

M. Dessaignes states, that when he gave a short account of the conversion of rough malate of lime into succinate of lime by spontaneous fermentation, he proposed to add to this first observation such facts as he might obtain from analogy. This research was considerably advanced when M. Liebig published a memoir on the same subject; some facts, however, were observed by M. Dessaignes which escaped the notice of M. Liebig.

M. Dessaignes employs rough caseine as a ferment; it is thoroughly mixed with water, holding either in solution or suspension the substance experimented upon, and the whole is to be exposed to summer heat for three weeks or a month. The author's experiments were performed upon perfectly pure neutral malate of lime, supermalate of lime, malate of potash, aspartate of potash, aspartate of lime, fumarate of lime, maleate of lime, and aconitate of lime extracted from the *Aconitum napellus*. All these salts are readily converted into succinate under the influence of the fermentation of caseine. Asparagin, under the same influence, begins by changing into aspartate of ammonia, and this is converted into succinate. In fact, if the fermentation is interrupted long before it is finished, a large quantity of aspartic acid is found in the liquor, as well as succinic acid.

The body which exists in the family of the leguminous plants, which has not hitherto been isolated, and is converted by germination into asparagin, is also susceptible of being converted into succinic acid. In fact, if the meal of peas be diffused through water for twelve hours, and then fermented, after having added chalk and filtered the liquor, a notable quantity of succinate of lime is formed. M. Dessaignes separately fermented legumin, the liquor from which it was precipitated, and also a nitrogenous body which has been noticed by M. Braconnot; by these means it was hoped that the substance which produces the succinic acid might be discovered. All these fermentations yield succinic acid in quantities which are certainly unequal, but the author has not yet terminated this part of his researches; the same acid was also produced by fermenting the emulsion of sweet almonds, separated from its oil and mixed with chalk. It appears, therefore, that the succinic fermentation occurs in nature as frequently as the acetic, metacetic, butyric, and valerianic fermentations.

He adds some observations on the isomeric acids with the formula $C^4 H^2 O^4$.

It has been stated above, that the fumaric, maleic and aconitic acids, are all of them convertible into succinic acid. This similarity of conversion is remarkable; for, on the one hand, the citrates of lime or soda fermented with caseine do not yield succinic acid, and on the other hand, the two acids derived from the malic acid are very clearly distinguished from aconitic acid by another metamorphosis. In fact, it was found that the bifumarate and bimaleate of ammonia, submitted to dry distillation, yield a substance much resembling in most of its reactions, but not identical with, the bimalate of ammonia produced under similar circumstances. This matter, by the prolonged action of hydrochloric acid, is converted into aspartic acid, absolutely the same as that obtained with malic acid. But the biconitate of ammonia and the biequisetate of ammonia, submitted to the same treatment, do not produce aspartic acid. The neutral malate of ammonia does not precipitate perchloride of iron, whilst the neutral aconitate and equisetate of ammonia do precipitate this salt. In the comparative examination which the author commenced of these three acids, he was readily convinced of the perfect identity of

the aconitic and equisetetic acids, and of the non-identity of the latter with maleic acid; but M. Dessaignes states, that the details which he could give on this subject are rendered useless by the recent publication of M. Baup thereon. The author concludes this notice by stating a method of obtaining, with asparagin, an aspartic acid which crystallizes in the same form as aspartic acid procured from the bimalate of ammonia. Heat to 392° F. aspartate of ammonia obtained from asparagin, till no more ammoniacal odour is perceptible, there remains a brown substance that is very slightly soluble, which, treated with hydrochloric acid, reproduces aspartic acid, crystallizing in short hard prisms, like those of the acid derived from the malic, maleic and fumaric acids.—*Comptes Rendus*, Septembre 16, 1850.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1850.

Chiswick.—September 1. Drizzly. 2—5. Very fine. 6. Clear and fine. 7. Frosty: very fine. 8. Cloudy. 9. Overcast. 10. Overcast: clear. 11. Foggy: very fine. 12. Foggy: fine: clear. 13. Slight fog: fine: clear. 14. Foggy: fine: clear. 15. Slight fog: cloudy. 16. Overcast: cloudy. 17. Clear and fine. 18. Fine: overcast. 19. Foggy: overcast: rain. 20. Cloudy: rain. 21. Boisterous: heavy rain at night. 22. Heavy rain. 23. Rain: lightning in the evening. 24. Cloudy: very fine. 25. Dense fog: very fine. 26. Overcast: constant heavy rain. 27. Fine: thunder and heavy rain in afternoon. 28. Clear: fine: clear. 29. Fine: rain. 30. Rain: rather boisterous: overcast.

Mean temperature of the month 54°·23

Mean temperature of Sept. 1849 57°·76

Mean temperature of Sept. for the last twenty-four years . 57°·23

Average amount of rain in Sept. 2·61 inches.

Boston.—Sept. 1. Cloudy. 2. Fine. 3. Cloudy: rain p.m. 4—6. Fine. 7. Cloudy: rain p.m. 8—12. Cloudy. 13—15. Fine. 16, 17. Cloudy. 18. Fine. 19. Cloudy. 20. Fine: rain p.m. 21. Cloudy: rain early a.m. 22. Fine. 23. Cloudy: rain p.m. 24. Cloudy. 25. Foggy. 26. Rain: rain early a.m. 27. Fine: rain early a.m. 28, 29. Fine: rain p.m. 30. Cloudy: rain a.m.

Applegarth Manse, Dumfries-shire.—Sept. 1. Fine harvest day. 2. Fair, but cloudy: threatening p.m. 3. Very fine: frost rime in the morning. 4. Frost rime a.m.: one slight shower. 5, 6. Fair and fine. 7. Fair and fine, though chill. 8. Fair and fine: milder. 9. Fair and fine: smelling of frost. 10. Fair and fine: cloudy. 11. Fair and fine: cloudy: very warm a.m. 12. Fair and fine: heavy dew: white rime. 13. Fair and fine: fine harvest weather. 14. Still fair and fine. 15. Dull a.m., but cleared, and was fine. 16. Fine, but colder. 17. Fine, but colder: mercury falling. 18. Fine, but colder: moon wading. 19. Fair, but threatening change. 20. Rain a.m.: rain also p.m. 21. Heavy showers all day. 22. Heavy showers a.m.: cleared, but moist. 23. Very fine, after showers in the night. 24. Fine harvest day. 25. Fair, but cloudy: cleared p.m. 26. Rain during night and morning: cleared. 27. Succession of heavy showers: flood. 28. Heavy rain during night: ditto day. 29. Rain and hail p.m. 30. Showers.

Mean temperature of the month 51°·65

Mean temperature of Sept. 1849 53°·5

Mean temperature of Sept. for the last twenty-eight years... 53°·17

Average rain in Sept. for twenty-three years 3·13 inches.

Sandwich Manse, Orkney.—Sept. 1. Drizzle: showers. 2. Rain. 3. Clear: cloudy. 4. Showers: damp. 5. Showers. 6. Fine. 7. Fine: aurora. 8. Fine: cloudy. 9. Fine: cloudy. 10. Cloudy: aurora. 11. Fine: fog. 12. Fine: cloudy: clear: aurora. 13. Fine: cloudy. 14. Clear: aurora. 15. Hazy: aurora. 16. Fine: hazy: fine. 17. Fine. 18. Fine: cloudy. 19. Cloudy. 20. Cloudy: drops. 21. Cloudy: rain. 22. Bright: rain. 23. Showers: cloudy. 24. Cloudy: clear. 25. Bright: clear. 26. Hazy: clear. 27. Bright: clear. 28. Rain: showers: aurora. 29. Bright: showers: clear: aurora. 30. Showers: aurora.

Days of Month.	Barometer.						Thermometer.				Wind.				Rain.										
	Chiswick.			8 $\frac{1}{2}$ a.m. Boston.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		8 $\frac{1}{2}$ a.m. Boston.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.						
	Max.	Min.	8 $\frac{1}{2}$ a.m.		9 a.m.	9 p.m.	8 $\frac{1}{2}$ p.m.	Max.	Min.																
1850. Sept.																									
1.	30.348	30.333	29.83	30.19	30.24	30.17		30.11	30.17		72	46	63	54 $\frac{1}{2}$	55	55	wnw.	w.	0.1	0.6					
2.	30.385	30.292	29.86	30.23	30.18	30.09		30.09	30.09		72	41	60	63	55 $\frac{1}{2}$	52	46	wnw.	w.		0.65				
3.	30.238	30.190	29.70	30.09	30.18	30.22		30.22	30.43		64	51	60	61 $\frac{1}{2}$	51	40	sw.	w-ne.		0.93					
4.	30.322	30.274	29.86	30.28	30.27	30.55		30.55	30.55		66	30	53.5	59	39 $\frac{1}{2}$	47	49	nnw.	e.	0.02	0.03				
5.	30.377	30.277	29.90	30.23	30.25	30.53		30.53	30.56		66	37	50.5	67	50	48	ne.	nw.			0.07				
6.	30.343	30.285	29.87	30.33	30.35	30.60		30.60	30.67		64	30	52	57	41	49	ne.	n.							
7.	30.401	30.357	30.00	30.40	30.40	30.47		30.47	30.51		67	33	52	56	43	53	48	e.	nw.	calm					
8.	30.446	30.388	30.03	30.42	30.37	30.49		30.49	30.45		60	36	52	59	40	52	49	n.	ne.	n.	0.03				
9.	30.367	30.297	29.95	30.30	30.25	30.38		30.38	30.34		61	47	52.5	61 $\frac{1}{2}$	35	52	48 $\frac{1}{2}$	ne-se.	se.						
10.	30.318	30.288	29.86	30.23	30.20	30.29		30.29	30.25		65	35	55.5	58	39	52	50	calm	sw.	se.					
11.	30.272	30.230	29.80	30.28	30.15	30.20		30.20	30.20		69	37	60	64	41	53 $\frac{1}{2}$	52	e.	se-ssw	s.					
12.	30.250	30.219	29.83	30.20	30.19	30.22		30.22	30.23		70	32	60.5	65 $\frac{1}{2}$	40	46	40	e.	se.	se-sw.					
13.	30.266	30.241	29.86	30.25	30.22	30.26		30.26	30.32		68	32	53	65	43 $\frac{1}{2}$	53	54	e.	se.	se.					
14.	30.239	30.221	29.80	30.25	30.25	30.33		30.33	30.32		59	40	55.5	59	38 $\frac{1}{2}$	54	48	e.	ene.	ne.	calm	0.01			
15.	30.252	30.247	29.86	30.29	30.29	30.33		30.33	30.38		65	52	59	58 $\frac{1}{2}$	47	52	49	e.	ene.	ne.	calm				
16.	30.284	30.251	29.88	30.32	30.30	30.38		30.38	30.37		67	51	59.5	58	41	53 $\frac{1}{2}$	49	e.	ene.	ne.	se.	0.00			
17.	30.290	30.197	29.88	30.28	30.17	30.31		30.31	30.19		69	39	56	61	35	55 $\frac{1}{2}$	51	e.	ene.	ne-sw	se.				
18.	30.073	29.888	29.62	29.98	29.80	29.78		29.92	29.78		69	46	59	60	44	53 $\frac{1}{2}$	54	e.	e.	sw.	ssc.				
19.	29.808	29.700	29.34	29.70	29.64	29.78		29.78	29.72		70	53	61	62	46	54	53	sw.	ese.	ese.	se.	0.02			
20.	29.632	29.463	29.18	29.54	29.52	29.75		29.75	29.72		66	53	62	58	52	55	54 $\frac{1}{2}$	s.	sw.	se-s.	se.	0.05			
21.	29.813	29.531	28.97	29.18	29.48	29.60		29.60	29.33		69	41	59	62	51	54 $\frac{1}{2}$	54	sw.	sw.	se-s.	ese.	0.21			
22.	29.889	29.834	29.40	29.65	29.77	29.53		29.53	29.62		71	46	55	59	49 $\frac{1}{2}$	55	53 $\frac{1}{2}$	ssw.	s.	sw.	ese.	0.06			
23.	29.803	29.753	29.37	29.80	29.75	29.75		29.75	29.82		71	51	60	56	51	54 $\frac{1}{2}$	53	e.	ene.	sw.	wnw.	0.23			
24.	29.724	29.715	29.26	29.70	29.66	29.79		29.79	29.75		67	45	62	55	43	55	51	sw.	ne.	sw.	ese.	0.04			
25.	29.803	29.682	29.27	29.64	29.53	29.66		29.66	29.61		68	48	56	60	39	54	52	sw.	w.	se.	se.	0.12			
26.	29.870	29.860	29.29	29.51	29.51	29.52		29.52	29.55		60	44	57	60	49 $\frac{1}{2}$	55	53	s.	sw.	sw.	se.	0.02			
27.	29.802	29.690	29.29	29.48	29.35	29.52		29.52	29.34		63	42	53	56	50	55	53	sw.	sw.	sw.	se.	0.10			
28.	29.867	29.782	29.23	29.38	29.50	29.09		29.09	29.29		63	42	52.5	57	45 $\frac{1}{2}$	50 $\frac{1}{2}$	50	w.	ws.	w.	ws.	0.35			
29.	29.846	29.380	29.32	29.50	29.23	29.33		29.33	29.34		62	42	51	56	44	50	44	sw.	s.	s.	sw.	0.29			
30.	29.359	29.266	28.80	29.17	29.09	29.28		29.28	29.37		59	40	49	54 $\frac{1}{2}$	41	49	48 $\frac{1}{2}$	w.	ws.	ws.	ne.	0.01			
Mean.	30.089	30.004	29.60	29.960	29.936	30.009		30.009	30.011		66.20	42.26	56.3	59.8	44.3	52.68	49.96					2.36	2.22	2.43	4.05

Fig 1.



Fig 2.



Fig 3.



Fig 4.



Fig 5.



Fig 6.

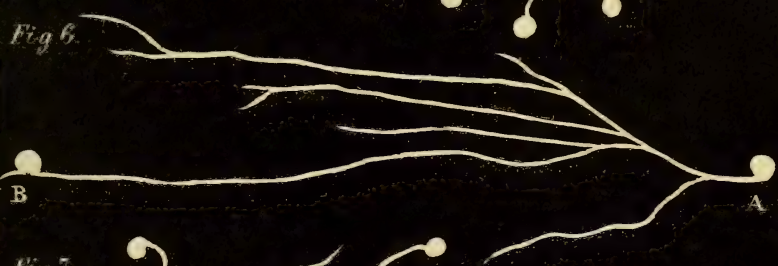


Fig 7.



THE
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LIV. *On the alleged evidence for a Physical Connexion between Stars forming Binary or Multiple Groups, deduced from the Doctrine of Chances**. By JAMES D. FORBES, Esq., F.R.S., Corresponding Member of the Institute of France, &c.†

1. **A**N opinion has long obtained amongst astronomers that the great number of cases which occur in the heavens of two or more stars being apparently very close to one another (constituting what have been called double, triple or multiple stars), constitutes of itself an argument for a more than *apparent* connexion between the members of those groups.

2. It is evident that two stars may constitute an *apparently* double star without any real proximity between them, merely because a line passing through the eye of the spectator and the nearer star may, if prolonged into space (no matter how far), pass somewhere near a second star, whose position would therefore seem almost to coincide with the first, although the distance which separates them might be indefinitely great. Such stars are sometimes said to be “optically” double. On the other hand, it may happen that the two stars are really

* This paper was commenced and a great part of it written in the month of May 1850, with the intention of its being read at the meeting of the British Association in August. Circumstances prevented its completion or revision; and what I had written in May I read to the Physical Section of that body on the 6th of August. The only addition then made was a summary of conclusions which I have inserted *verbatim* in the present paper, and which form the *first four* of those given in § 37. Those articles which have been incorporated into this paper from the original draft without any, or only slight verbal alterations, are distinguished by having the current numbers prefixed to them inclosed in brackets, thus [8].

† Communicated by the Author.

Phil. Mag. S. 3. Vol. 37. No. 252. Dec. 1850. 2 D

as well as seemingly near, and may act upon one another by their mutual attractions, after the manner of sun and planet. Such stars are called “physically” double.

3. Now it is an interesting fact, that long before we had the convincing proof that many binary stars are “physically” as well as “optically” double, which was afforded by the splendid discovery of Sir William Herschel that such stars do in many cases describe orbits round their common centre of gravity, the Rev. John Mitchell, a philosopher of great and original talent, had inferred inductively the probability that such a physical connexion existed, merely from their closeness of apparent position, and without any proof of their exerting a sensible attraction, or other reciprocal influence. Having arrived himself inductively at the conclusion, he endeavoured to put his grounds of belief into a mathematical shape, which should compel the assent of all reasonable persons, by showing the great numerical probability which exists (according to him) against the contrary supposition, namely, that such stars are only “optically,” not “physically” double.

4. Mitchell’s argument is contained in a paper, remarkable on many other accounts, entitled “*An Inquiry into the probable Magnitude and Parallax of the Fixed Stars from the quantity of Light which they afford to us, and the particular circumstances of their situation,*” printed in the Philosophical Transactions for 1767*. Amongst other results he obtains this one (so often quoted since), that “the odds are near 500,000 to 1, that no six stars out of 1500 equal in splendour, scattered at random in the whole heavens, would be within so small a distance from one another as the Pleiades are†.”

5. The principles of reasoning employed in this paper (to which we shall presently revert), and the results contained in it, have been sanctioned by the authority of writers of the very highest eminence, whether on astronomy or on the theory of probabilities; who, since the time of Mitchell, or for more than eighty years, have adopted them without (so far as I know) a dissentient voice; and the extent to which the principles of Mitchell have been implicitly admitted by his successors may be understood from the following statement, resting on the very highest living authorities on sidereal astronomy. M. Struve, director of the Pulkowa Observatory, “calculates the odds at 9570 to 1 against any two stars, from the 1st to the 7th magnitude inclusive, falling (if fortuitously scattered) within 4" of each other. Now the number of such binary combinations actually observed at the date of this calculation was already 91, and many more have been added to the list.

* Vol. lvii. p. 234.

† Ibid. p. 246.

Again, he calculates that the odds against any (two?) such stars fortuitously scattered falling within $32''$ of a third, so as to constitute a triple star, is not less than 173,524 to 1. Now, four such combinations occur in the heavens." Sir John Herschel, from whose *Outlines of Astronomy* I take this statement of Struve's results, adds, "the conclusion of a physical connexion of some kind or other is therefore unavoidable*."

6. Now I desire it to be most distinctly understood, that the argument which I have to state is not meant to controvert the truth of the general result at which Mitchell and Struve arrive, namely, that the proximity of many stars to one spot, or the occurrence of many close binary stars distributed over the heavens, raises a *probability*, or rather we would call it an *inductive argument*, feeble perhaps, but still real, that such proximity may be actual, not merely apparent; but I deny that such probable argument is capable of being expressed numerically at all; and I hope clearly to prove, that it has no absolute and compulsory form addressing itself alike to all understandings and to all capacities, and to persons ill and well-informed alike; but that the grouping of stars is like any *phenomenon* occurring in physical investigations, which suggests further inquiry; which points at a result not improbable, but requiring to be inductively established by bringing together other considerations, whose accumulation may impel conviction upon minds capable of estimating logical evidence, although this evidence is no more capable of being expressed by numbers than is the evidence for the truth of the theory of gravitation.

7. These convictions were stated in a short letter addressed (principally, however, in the language of doubt and inquiry) to the Editors of the *Philosophical Magazine and Journal*, which was published in that periodical for August 1849. I had not then read Mitchell's paper, nor had I studied his argument; my incredulity was based on the general logical ground that the data, as stated, do not contain the elements for arriving at a numerical estimate of the probabilities for or against such results. Having since considered the matter at

* *Outlines of Astronomy*, p. 564. 8vo. Longmans, 1849. I have thought it quite necessary to recite this passage, which includes the reasoning, or results of reasoning, to which I object; because I have been supposed to refuse my assent to reasonings by no means of the same class, or liable to the same objections as those contained in these sentences, which were specifically quoted in my letter to the Editors of the *Philosophical Magazine* mentioned in art. 7. M. Struve and Sir John Herschel have only adopted and extended the conclusions implicitly received by many illustrious predecessors, and are of course not more individually answerable for them than they.

leisure, and studied Mitchell's arguments, I am confirmed in my conviction of the fallacy of the numerical estimates of the so-called proof of the physical connexion of double stars. I also believe that the refutation of the fallacy (if such it be) is of great importance; first, because any abuse of the mathematical sciences, such as to give definite demonstrations and results where no such demonstrations and results can, in the nature of things, be legitimately attained, tends to weaken our confidence in mathematical conclusions generally; and secondly, and still more strongly, because the evidence for final causes is so peculiar, and its discussion so confessedly difficult and delicate, that all attempts to base the proof of design on strictly *à priori* and geometrical grounds, and to estimate it numerically, should be received with scrupulous caution. These are questions in regard to which our suspicions cannot be too sensitively awake. The natural philosopher, in particular, who must rely at every instant on the certainty of mathematical proof, must not harbour even a doubt of its accuracy without probing the matter to the bottom. After doing so, should he remain unsatisfied, he must feel it a duty to communicate to others the result at which he has arrived, for correction, if need be, but in any case regardless of the authority of names which may be arrayed on the other side.

[8.] Mitchell's argument, stated in the Philosophical Transactions for 1767, to prove the proposition that when two or more stars *appear* very near one another in position they are not merely apparently but really so, "either by an original act of the Creator, or in consequence of some general law, such perhaps as gravity," is to the following effect. Let a star be given in (apparent) position, and call it A. Let other similar stars, B, C, D, &c., be "scattered by mere chance as it might happen" over the heavens. "Now it is manifest," says Mitchell, "upon this supposition, that every star being as likely to be in any one situation as another, the probability that any one particular star should happen to be within a certain distance (as, for example, one degree) of any other star would be represented by a fraction whose numerator would be to its denominator as a circle of one degree radius to a circle whose radius is the diameter of a great circle (this last quantity being equal to the whole surface of the sphere), that is, about 1 in 13131." This ratio expresses the probability (according to Mitchell), that if there were but two stars in the heavens, they should be found (supposing them placed "at random") within 1° of one another, and $\frac{13130}{13131}$ will express the probability that they shall not be so near.

9. Next suppose that there are n other stars in the same category with the given star A, and the independent probability that each shall *not* be within a degree of it to be that just found, the probability that these n events shall concur, or that no one star shall be within 1° of A, will be expressed by multiplying the several probabilities together, or by the number $\left(\frac{13130}{13131}\right)^n$.

“And farther,” adds Mitchell, “because the same event is equally likely to happen to any one star as to any other, and therefore any one of the whole number of stars n might as well have been taken for the given star as any other, we must again repeat the last found chance n times, and consequently the fraction $\left(\frac{13130^n}{13131^n}\right)$ will represent the probability that nowhere in the whole heavens, any two stars amongst those in question would be within the distance of one degree from each other; and the complement of this quantity to unity will represent the probability of the contrary*.”

10. So far as I see, the first part of the argument (in § 8) involves an error, or rather two errors in principle; the second part (contained in the last paragraph) an error in detail, which vitiates the numerical results arrived at. The last, as being of little consequence if the first be established (which would overthrow the entire calculation), I shall refer for discussion to a note at the end of this paper†.

11. It is evident that the argument for the physical connexion of double stars, derived from the Theory of Chance, depends entirely upon our admission of the primary result, that two given stars cannot be within one degree of one another without inferring a reason for it sufficiently powerful to balance the adverse probability of 13131 to 1. All the rest follows, or may be made to follow, as a matter of course.

[12.] Let us look at the case straightforwardly. Suppose two luminaries in the heavens, as two dots on a sheet of paper of known dimensions and figure. Is it or is it not common sense to say, that the position of one luminary or one dot being given, the law of “random scattering” can assign any other position in the field as more or less probable or improbable for the second. If one star be in one pole of the heavens, is the most probable position of the second in the opposite pole, or is it 90° from it, or is it 30° , or is it in any assignable position whatever? I think not. Every part of the field, even that close to the star A, is an equally probable allotment for the star B, if we are guided by no predetermining hypothesis

* Mitchell, pp. 244, 245.

† See note A.

or preconception whatever. Whence, then, this astounding probability of 13130 to 1 that B shall not be within a degree of A? If it express anything, what does it express?

[13.] What the probability $\frac{13130}{13131}$ expresses may be thus illustrated*. Suppose it to be known that two comets exist at once in the sky, of which one is telescopic, and its position has not been indicated. The probability in question represents the *expectation* which will exist in the mind of a person possessed of this partial information, that if he takes a telescope with a field of 2° in diameter, and fixes the cross wires upon the larger comet, the telescopic one shall *not* be somewhere in the field, which includes $\frac{1}{13131}$ of the whole spherical area. Mitchell's probability is therefore a significant number; but it belongs to an estimation of chance totally distinct from that to which he applied it; for it is evident, on reflection, that the ratios or probabilities of which we have been speaking have no *absolute* signification with reference to an event which *has* occurred, such as the distribution of stars on the celestial sphere, or the throwing of any number of dice. They represent only the state of *expectation* of the mind of a person before the event has occurred, or having occurred before he is informed of the result; for *that* can make no difference on the grounds of his expectation of a given event. The dice are thrown, the result is determined; but the *expectation* of a person as to that result is unchanged until he casts his eyes on the table, at which instant expectation, and all reasoning founded on chance, random, hazard, or by whatever name we choose to call it, vanishes, and Certainty takes the place of Doubt. The possible combinations, in respect of position, of even a limited number of stars is evidently infinite; and it does appear to be an illegitimate application of mathematics to infer the antecedent probability of any *one* arrangement which experience presents us with, since but one arrangement can exist at a time. I suspect that as we narrow the field of speculation by withdrawing every inference from assumed causation, we shall have very little, if anything, to reason about. I shall give an instance in illustration of my meaning.

[14.] Suppose a person wholly unacquainted with the game of chess to notice a chess-board with two or more pieces standing upon it; will several pieces standing near to one another on one part of the board give him any idea of their places being

* This illustration is different from that given in the original paper.

other than accidental? Will, for instance, the position of two pieces (even if they were the only two on the board) occupying adjacent squares, appear to him to be more the result of design than if they occupied opposite corners of the board? I altogether disbelieve it: and I am disposed to say that even if four, five, or more pieces stood close together, the idea would arise in the mind that they were placed there by some localizing cause, *not because mere chance has any distributive tendency* which leads us to suspect that it has been interfered with, when we observe a conglomeration, but from *the impatience of the mind of causeless phenomena*. And since the grouping together of pieces in one part of the board is a phænomenon striking to the sense and intellect, just as a uniform spacing of them over the board would be also such a phænomenon, the mind hastens to conjecture some cause which may account for that localization, *not because the phænomenon rebels against the laws of hazard*, but because it affords a kind of salient point to which the reasoning faculty may attach some threads of argument, however feebly united. Thus, *from the belief of causation in the abstract*, some faint probability arises in the spectator's mind that the arrangement of the pieces in question was not (as it might have been) purely accidental.

[15.] But now let an experienced chess-player step in. He sees not merely certain pieces in certain places, but, knowing the rules of the game, he can say, almost at a glance, whether the pieces placed before him were placed *by design* or otherwise, whether, in short, he sees a step of the game. If he does, his estimate of the grouping not being the result of hazard, rises to the very highest degree of probability, and higher in proportion to the number of the pieces and the critical character of the positions. Yet his probable estimate, like the last, is only a relative estimate, incapable of being expressed accurately by numbers: if it were otherwise, the numbers could only express the impression made by the combination on his own individual mind.

[16.] I am persuaded that the case of the stars is very similar. A group like the Pleiades attracts notice. It is a *phænomenon*. The mind hastens to attempt to assign by induction a cause for it. The symmetry of certain nebulous and multiple stars has a similar effect on the reasoning faculty. A star, for example, exactly in the centre of a nebulous mass nearly circular, has a position neither more nor less probable than any other position within or without the nebula. It is the *inductive bias* of every rational mind which thrusts into our reasonings the notion of *cause*, and forbids us to think that accident might have placed it there as well as anywhere else. A happy and

just propensity of man's mind it is to reason thus; but the science of numbers refuses its aid to estimate the arithmetical force of such arguments.

[17.] The *impression of design deduced from symmetry* is one of the most striking which it is possible to suppose; and yet, as far as I know, it is impossible to represent the improbability that it should be the result of chance. In fact, a symmetrical arrangement, so far from being an improbable one, would, if Mitchell's argument were founded in reason, appear the most conformable to that most hazardous of the laws of hazard, the law of *sufficient reason*. This I shall endeavour presently to illustrate further. Meanwhile I observe that a symmetrical arrangement of points placed "at random," being one of many random ways of placing those points, all equally probable, would appear to have neither preference nor the contrary on the doctrines of chance*. And here we also notice the important distinction already referred to, between *an accomplished fact* and *an event which is anticipated*, though there would be millions or even infinity to one against the probability of a symmetrical arrangement;—*once made*, it takes a position (by the laws of *mere chance*) as a result as probable as any other arrangement which might have presented itself. But the eye of reason can never acquiesce in this conclusion, from the simple consideration of antecedent probabilities. † Were the stars (for example) disposed in square or hexagonal order over the entire sky, could we for a moment tolerate the conception of a "random scattering," which (at first sight) appears consistent enough with their actual distribution; and does not the occurrence of condensed groups or pairs of stars in some places, and of great vacuities in others, seem to be rather the rule of chance than its exception?

[18.] It is not many years since the idea of the foot-prints of animals, which walked over the then soft surfaces of the older rocks ages before man's creation, being yet in evidence, was scouted as absurd: and it might have been so still had the proof been to be sought solely in the clearness of the foot-prints being those of an animal, differing as they no doubt did from those of any species now existing. But the *symmetry*

* "Les combinaisons régulières n'arrivent plus rarement que parce qu'elles sont moins nombreuses. Si nous recherchons une cause, là où nous apercevons de la symétrie; ce n'est pas que nous regardions un événement symétrique comme moins possible que les autres; mais cet événement devant être l'effet d'une cause régulière ou celui du hasard, la première de ces suppositions est plus probable que la seconde."—Laplace, *Essai sur les Probabilités*, p. 19.

† The latter part of the paragraph did not stand part of the original draught of the paper.

with which the prints succeeded one another on the surfaces of the sandstone, reproducing group after group of steps in two rows marked by the irregularity of interval characterizing the motion of quadrupeds, furnished an argument, geometrical no doubt, but incapable of numerical definition, which, when combined with the likeness of the individual marks to the foot-prints of certain animals, raised the evidence to the amount producing conviction.

19. The arguments and illustrations now adduced to invalidate the conclusion of Mitchell and his followers, that it is possible to assign *numerical* probabilities for or against the present distribution of the stars, or of any part of them, or even of two or three individual stars especially selected, may I think be reduced to the shape of two main objections to the principle and its primary results, either of which would, I conceive, be fatal to Mitchell's conclusions. I shall state them in the order in which they appear most likely to be at once assented to. I shall first state the objection to the application made of the principle of "random scattering;" I shall next state an objection to the fundamental definition or implied axiom on which the whole argument is based.

20. FIRST OBJECTION.—*The doubt existing in the mind of a reasonable person, whether an event still future, and which may happen many ways, shall occur in a particular given way, is erroneously considered as equivalent to an inherent improbability of its happening, or having happened, in that way.*

21. Thus, 10,000 balls numbered consecutively being placed in a bag, the antecedent chance or expectation of drawing a given number is $\frac{1}{10000}$; but it is not the less necessary that a specific number be drawn. It was 9999 to 1 that the number 65 (suppose) should not be drawn; but 65 being *de facto* the number drawn, every vestige of improbability concerning it vanishes. Has not every fall of two common dice an antecedent probability of 17 to 1 against it if the numbers turned up be different*, and 35 to 1 if they be *doublets*? But have these probabilities any longer a meaning after the dies are cast and the numbers read? We then see one individual result neither more nor less likely in itself to happen than any other individual result. Now, to apply this to the case of double stars, we must take an illustration already made use of†

* For out of thirty-six possible cases, two cases satisfy the condition; for instance, three shown by the 1st die and five by the 2nd; or else five by the 1st and three by the 2nd; in the case of doublets the throw of each die is specifically determined.

† See note A.

as conformable to Mitchell's fundamental supposition. To express the antecedent improbability (which Mitchell confounds with an *inherent* improbability) of two stars falling within 1° of each other, or what is the same thing, of a star falling within a circle 2° in diameter described round the position of any other star as a centre, it is shown in Note A at the end of this paper, that we may imagine as many dice to be thrown at once as there are stars to be considered, and each die to have as many faces as there exist spaces 2° in diameter in the whole heavens (which is 13131), then the occurrence of any doublet in such a throw of dice is equivalent to the duplication of any star by another within 1° of it; and the chance against the occurrence of doublets is (on Mitchell's theory) the chance against the duplication of any star. The chance of throwing a doublet with 100 dice may thus be shown to be about $\frac{2}{5}$. If there were only two dice the chance would

be just $\frac{1}{13131}$, the reciprocal of the number of faces on each*.

But though there are 13131 different throws for one which is a doublet, there is no more improbability in that than in any other given throw, and therefore, although the appearance of a series of doublets produce a reasonable suspicion of an interfering cause, the grounds of that suspicion can by no means be expressed by the numerical antecedent improbability of the event.

22. A little consideration will show that no event, tolerably complex (such as the distribution of stars, supposed by chance, or the fall of many dice at once), can occur without the chances, reckoned on the principle of expectation, being perfectly enormous against it, and giving to it the air of an extravagant *à priori* improbability. The improbability, for instance, of a given deal producing a given hand at whist is so immense, that were we to assume Mitchell's principle, we should be compelled to assign it as the result of an active cause with far more probability than even that found by him for the physical connexion of the six stars of the Pleiades.

23. A natural but very important error has been made in the use of the term "most probable result." A result which, on the long run of many trials made under the same circumstances, may be expected to occur oftenest, is truly called the

* Thus with common dice, having six faces, the number of ways the dice may fall is thirty-six: but six cases give doublets; the chance of throwing doublets is therefore evidently $\frac{1}{6}$.

most probable result; and yet the improbability of that result (though less improbable than any other given result) may be very great. At the game of whist the most probable result is, that each player should have as nearly as possible an equal number of cards of each suit in his hand; and if the cards admitted of equal subdivision, an *exactly* equal number: yet such a result is manifestly most improbable. Its very occurrence in all the four hands would provoke the suspicion of an interference with the common course of chance, at least as much as (let us say) the total absence of one suit in one hand would do. If we then were witnesses of only a single game, how preposterous would be the idea of subtracting the numbers of any suit in any hand from the "most probable numbers," and declaring that the difference is a proof that the deal has not been fair! *

24. I have thought it worth while to test a little by simple experiment the differences to which "mere chance" gives rise in the grouping of bodies dispersed over a surface, by a method of "random scattering" which I conceive to be as nearly as possible analogous to Mitchell's idea of chance as affecting the placing of the stars. I had referred in my letter in the *Philosophical Magazine* to the pretty fact noticed by Mr. James Naysmith, that the sparkings of a brush loaded with white paint produced a distribution of spots on a dark ground which represented exceedingly well the general aspect of the grouping of the stars, including many double and multiple specks; and I quoted this partly as an illustration, and partly as an argument against the assumed improbability of producing such combinations by chance. The experiment having been objected to as not free from causes of doubt, I have made some trials in the following way. I placed a chess-board, having, as usual, sixty-four squares, on the floor, and I provided a large sieve into which I put a quantity of grains of rice, which did not fall through the sieve until it was somewhat shaken. I then shook the sieve at a considerable height above the chess-board until it was pretty well scattered over with grains. And the "random scattering" of these grains was much increased by the circumstance, that the board was so elastic that every one of the grains rebounded from the surface and fell once or oftener, so that their mutual positions were thus as much as possible interchanged.

25. The following diagrams contain the results of five experiments, the number of grains which fell on each of the sixty-four squares being counted and registered. Only these five experiments were made; none have been rejected.

* Such an argument has actually been used to prove the physical connexion of double stars.

Experiment I.

Sums.

	4	3	3	6	2	4	2	1	25
	4	4	4	3	1	4	3	1	24
	6	2	3	2	3	5	3	2	26
	3	0	4	3	4	7	1	3	25
	0	4	4	4	0	3	4	4	23
	3	2	3	3	2	5	5	5	28
	2	3	3	0	3	5	1	1	18
	1	3	3	2	1	2	0	2	14
Sums	23	21	27	23	16	35	19	19	

Results.

Total number of grains	183
Average per square	2.9
Greatest number in one square . . .	7
Squares vacant	5
Squares with one grain only	8

Experiment II.

Sums.

	2	7	4	1	6	5	5	3	33
	5	3	3	5	6	7	2	8	39
	7	6	3	2	7	6	1	4	36
	4	8	8	3	4	5	2	4	38
	0	5	6	7	4	2	3	7	34
	1	6	3	4	6	8	5	2	35
	2	4	4	7	5	3	4	8	37
	2	4	5	5	3	3	3	4	29
Sums	23	43	36	34	41	39	25	40	

Results.

Total number of grains	281
Average per square	4.4
Greatest number in one square (occurs four times)	8
Squares vacant	1
Squares with one grain only	3

Experiment III. Sums.

1	1	3	0	3	0	4	0	12
2	0	3	4	0	1	0	3	13
3	3	3	2	3	3	9	2	28
2	2	2	1	0	1	5	3	16
1	2	3	1	3	1	3	2	16
1	1	5	1	3	3	3	3	20
1	1	4	4	2	2	3	1	18
1	2	0	0	3	4	0	1	11
Sums	12	12	23	13	17	15	27	15

Results.

Total number of grains	134
Average per square	2.1
Greatest number in one square	9
Squares vacant	10
Squares with one grain only	16

Experiment IV. Sums.

2	2	6	6	4	1	2	4	27
3	5	3	6	1	6	1	6	31
4	3	4	7	4	4	6	3	35
3	6	10	5	4	7	2	7	44
2	4	10	6	10	7	2	2	43
2	3	4	13	5	10	8	3	48
2	1	6	6	4	2	8	1	30
6	4	6	4	2	3	2	12	39
Sums	24	28	49	53	34	40	31	38

Results.

Total number of grains	297
Average per square	4.6
Greatest number in one square	13
Squares vacant	0
Squares with one grain only	5

Experiment V.

Sums.

	3	1	5	2	1	1	2	2	17
	1	3	0	1	4	6	5	0	20
	4	4	2	2	3	5	3	2	25
	2	2	7	0	3	5	3	1	23
	3	3	2	2	1	3	3	3	20
	4	3	9	2	1	2	3	4	28
	0	5	6	4	4	2	5	4	30
	5	5	3	3	5	2	2	3	28
Sums	22	26	34	16	22	26	26	19	

Results.

Total number of grains	191
Average per square	3.0
Greatest number in one square	9
Squares vacant	4
Squares with one grain only	8

26. *Observations on the preceding experiments.*

(1.) I will first of all remark, that though the idea was to hold the sieve in such a manner as to bear uniformly over the whole board, there appears in most of the experiments a slight tendency to greater concentration towards the centre, as is shown by the horizontal and vertical summations which I have added. This might have been probably corrected by such an alteration as filling the sieve more completely; but an examination of the contents of the squares, and the irregularities marked by contiguous squares, show that this cause has but slightly operated in producing the observed inequalities of distribution. I conceive, then, that under the given conditions

of scattering, the inequalities of dispersion from one square to another due to "accident" are well represented, and can be ascribed to no assignable "cause."

(2.) In these experiments we observe that the most loaded squares contain from nearly two to more than four times the *average* number of grains, whilst in four out of five experiments one or more squares were vacant.

(3.) It is quite certain that, had the spaces been smaller and more numerous, the inequalities would have been still greater; for in this form of experiment extent of surface has an equalizing tendency.

(4.) In all the experiments we observe squares having heavy and light loads very much intermixed. In three cases out of five the heaviest squares are adjacent to others altogether blank.

(5.) If we were to take any one of these experiments, and attempt to calculate the antecedent probability of the grains so arranging themselves on Mitchell's supposition, we should unquestionably find numerical chances far greater against these configurations being the result of accident, than those on which we are told that we form our most certain ordinary judgements. Thus if an experimental argument may be admitted, the reasoning of Mitchell and his followers is altogether fallacious; for we find that it would lead to results extravagantly absurd in cases admitting of no doubt.

27. It will perhaps be said, that though Mitchell's reasoning was wrong, and his calculations of the chances adverse to any particular arrangement of the stars altogether false and exaggerated, the doctrine of probabilities has other and special methods applicable to such cases as experiment has here been employed to illustrate. If it be so, they have never been applied to the astronomical question before us,—the question whether the double stars are so "optically" or "physically;" and consequently we are not required to refute investigations which have never been made. The argument which has been now used appears to be sufficient to prove that Mitchell's attempt to express the numerical probability of a causal connexion between the pairs of double stars is a complete failure. I think, too, that I have demonstrated that the *presumption* for such a connexion, which unquestionably arises when the number of close double stars is considerable, arises in the mind in the same way as the belief in the physical connexion of many phænomena of nature which can only be treated inductively, and whose ultimate probability is altogether incapable of estimation by numbers. I might therefore close my paper here. But I do not think it right to omit a conside-

ration which seems to me not unimportant with reference to the theory of causation treated by the doctrine of chances, though less obvious, and therefore perhaps less easily assented to at first sight, than the objection already made. It aims at showing that Mitchell's axiom or postulate is inconsistent with any idea which we can form of a "random scattering." As it is difficult to state what these words really mean, we shall frame our objection so as to rest entirely upon what they obviously do *not* mean.

28. SECOND OBJECTION.—*To assume that "every star is as likely, not hypothetically, but actually, to be in one situation as another" leads to conclusions obviously at variance with the idea of random or lawless distribution, and is therefore not the expression of that Idea.*

29. Let me first quote the words in which Mitchell states the principle of his argument (which principle has certainly been accepted down to this time). "Let us examine," he says, "what would have been the least apparent distance of any two or more stars, anywhere in the whole heavens, upon the supposition that they had been scattered by mere chance, as it might happen. Now *it is manifest, upon this supposition*, that every star being as likely to be in any one situation as another, the probability that any one particular star should happen to be within a certain distance (as, for example, one degree) of any other," and so forth, as quoted in § 8. I think that it is impossible to doubt that Mitchell meant to say, that the equality of chance referred to exists in the nature of things, in the same sense exactly as we admit an *actual* equality of chance that any one side of a well-poised die shall turn up,—as well as a *hypothetical* equality of chance that a given side shall turn up independently of our knowledge of whether the die be loaded or no. In treating of our first objection, we have shown that Mitchell and his followers have confounded the measure of *hypothetical* antecedent probability of a given result with a probability in the nature of things, or an actual probability, and have used the measure of the former (which we admit may be found correctly) for the measure of the latter. The Idea from which Mitchell starts is "scattering by mere chance," the application of the idea to the special case is "that every star is as likely to be in any one situation as another." The two things are assumed to be equivalent, the effect of the one supposition is the same as the effect of the other, and either is to be compared with the effect observed; and the probability or improbability of the Idea is to be thus tested. I believe that Mitchell held, and that most of those who have followed him hold this to be the true basis of the

argument; since a hypothetical probability or *expectation*, based on ignorance, and failing the moment that it is confronted with a *fact*, can hardly be seriously claimed as a source of Knowledge. Let us see, then, to what this definition of "scattering by mere chance as it may happen" must necessarily lead.

30. It appears from the reasoning already cited from Mitchell's paper, that, however numerous may be the stars included by the terms of the problem, the approximation of *any two* of them within a certain distance of one another, affords a presumption which increases in force *without limit* as the distance diminishes*, that such an approximation has been the result of a specific cause, and is not casual, a result surely inadmissible. The argument therefore for the physical duplicity of two stars, because their apparent distance is less than a given number of degrees or minutes, shows that there must be a limit to the argument, or a certain distance at which no such conclusion would be deducible. If no one star deviated from the average distance which each star in the heavens may have from its neighbours, no argument could be drawn one way or other for the physical connexion of two stars, since there is nothing in respect of juxtaposition to distinguish these two stars from the entire mass of stars which we suppose to be spaced in a perfectly uniform manner. Let us conceive the stars to be in fact quite symmetrically distributed over the sky. Mitchell's argument for duplicity is derived from comparative smallness of angular distance; but if the angular distance of each star from its neighbours be throughout the same, no argument can be applied to one star which will not be equally applicable to every other, and consequently the numerical argument for physical connexion or prevailing cause will cease the moment that a distribution perfectly symmetrical is supposed, a result too absurd to require refutation.

[31.] It is in reality *explicitly assumed* as the basis of the argument, that by the law of mere chance, scattering as it might happen, the chance of finding one star in a given space is proportional exactly to the space included in the imaginary limit. When there are many stars in question, the chance of finding each one in a given space is still stated to be as the space considered, and it is stated as an improbability that two stars should be found crowded into one space, or three stars into two spaces, or four stars into three spaces, and so forth. Thus our random law appears to indicate a *preference* for

* For, as the area which includes the two stars diminishes, the fraction in § 8, which expresses the improbability of their both being contained in it, approaches indefinitely nearer to unity.

equal spacing, since unequal spacing corresponds to a diminished probability of occurrence.

32. For further illustration of Mitchell's principle, let the heavens be divided into p equal areas, and let the fall of a die having p sides indicate the allotments into which a given star is expected to fall "at random," there being as many dice to throw as there are stars to distribute, then the question resolves itself into this:—"Of all GIVEN possible distributions to assign that in favour of which the *à priori* probability is greatest." I answer, *that a UNIFORM (and therefore generally symmetrical) distribution is more probable than any other given (i. e. observed) distribution.* For, to recur to the case of the dice, let there be 100 dice, each having 100 faces inscribed with the uniform series 1 100. It is no doubt very improbable, *à priori*, that the 100 dice thrown at random shall turn up exactly the arithmetical series 1 . . . 100; but it is *at least twice as probable as any other DEFINITE supposition* of distribution of numbers that can be made;—as for instance that the number 100 should be wanting, and that there should be two 99's. And the proof of this is sufficiently simple; the reason is the same as that there is a less chance of throwing given doublets than a given pair of numbers not alike, as A, B. For there are two combinations, A, B, and B, A, in favour of the latter event, but only a simple combination, as A A, in favour of the former*. Now the case of the 100 dice showing 100 consecutive numbers, represents the case of 100 stars, one of each of which falls into each of 100 different allotments into which the sky may be divided. And this is evidently a symmetrical distribution, or the nearest approach to symmetry which is geometrically possible. Hence, if Mitchell's rules of reasoning be applicable in this case, the probability is greater for a distribution of the stars having occurred "by mere chance, as it might happen," if such distribution were (as an observed fact) mathematically symmetrical, than if any given deviation from symmetry were observed in nature. I think that it cannot be necessary to insist upon the error of a conclusion so repugnant to common sense†.

* See note B.

† When I stated this result as that at which I had arrived in disproof of Mitchell's theory, at the Meeting of the British Association in Edinburgh, an able mathematician present entered into a calculation to show that the uniformity of distribution of bodies falling at random would not be the most probable result. Now the truth or error of his conclusion depends upon the use of the words "most probable." If by "most probable" we mean that a uniform distribution, or (as in the case we have taken to refer to) the turning up of all possible faces of the dice, is meant, the probability in favour of it compared to all other possible cases *taken together*, which might occur,

33. Assuming that this conclusion will not be contested,—that geometrical symmetry will never be accepted by a reasonable man as a proof that hazard has prevailed instead of design, “accident” against “cause,”—we are necessarily led to infer that the vague premiss of “scattering by mere chance as it may happen,” includes, as an essential conception, a sphere of possibilities wider than that of throwing dice in the manner supposed; and though we may be at first sight reluctant to admit this, I conceive that the *reductio ad absurdum* proves the necessity of it, and I shall endeavour to explain the kind of enlargement of the notion of chance or random which I conceive will meet the case.

34. It is plain from Art. 32, that there does exist an *actual* probability of symmetry when the chance of a given fall of dice is considered against the chances of any other given fall. *But there is something assumed as previously known or tried.* That something is the perfect indifference of the dice. This may be either assumed as a fact—suppose upon the information of the maker—or it may be ascertained by experiment. In the latter case it can only be known by the long run of a vast number of trials, proving that all faces turn up indifferently in the long run. But in the case of the stars we have witnessed no such series of experiments. The *actual* probability of the number of the stars being as the area considered, is here equivalent to the equality of balance of the dice in the last problem. Neither dare be assumed where the premisses are utterly vague. The result of many trials in the one case would be the tendency of all the numbers on the dice to turn up equally; in the other, the equality of interspacing of stars; each a most unlikely event, considered as a solitary result, yet

my mathematical friend was undoubtedly quite right. But if it mean the probability of the *given* symmetrical result, compared to the probability of some other *given* result not symmetrical, I hope to have shown that he is as certainly in the wrong. Now it is with a case *in esse*, an event which *has* occurred, not with a case *in posse*, an event which may or may not occur, with the previous probability for which we are here concerned. To take a very simple illustration. The chance of throwing heads and tails being reckoned to be precisely equal, it is yet *absolutely* very unlikely as a predictable result that on 1000 throws there should be exactly 500 heads and 500 tails. We know in fact that there are almost forty chances to one, that *some one* of the other possible events shall happen instead of the required one (Galloway on Probability, p. 134); but if we had seen the experiment tried, and had seen 499 heads and 501 tails as the result, the individual result of absolute equality is more probable (upon obvious grounds) than the individual result that there should be any given discordance; for the fact that such a preponderance of probability for heads or for tails existed, would be tantamount to an assertion that the chance was not indifferent, but that the coin had really a bias, which is contrary to the hypothesis.

in the nature of things more likely than any other event which could come singly under our notice. If we could form an idea of utter confusion or material *chaos*, it would not surely be of anything in which *uniformity* would have a conspicuous share; it would rather be the most heterogeneous variety in number and paucity, bulk and figure, density and rarity. It appears to me to be as arbitrary to assume this uniformity or want of bias, as it would be in attempting to reason about the properties of a mass whose figure is unknown, that it must be a sphere, because, in our ignorance of how it was formed, we can assign no reason why one of its diameters should be greater or less than another.

35. How then are we to enlarge the idea of "random" so as to meet the case? We must suppose that the dice have been formed out of non-homogeneous matter; that they are biassed in every possible way; that out of a great number of sets of such dice we are shown the result of the fall of one set only. Here we have no ultimate or ideal distributive tendency of chance to look to, because the result to which many experiments with one set of dice would tend, would not be that of uniformity. The bias is unknown; it is itself a condition of "chance" of a higher order.

36. Yet notwithstanding, with such dice two consequences follow. (1.) The final result of many throws, each with a different set of such biassed dice, would tend to uniformity, because the chance of the bias would ultimately run through all the possible cases. (2.) The *expectation*, or hypothetical probability of a given number turning up at any throw, would be exactly the same as if the dice were known to be unbiased.

37. In like manner the experiments with the sieve dropping grains of rice in § 25 can only be considered as in a very limited degree the representation of the phænomenon of "random scattering," though they approach as nearly to Mitchell's theory as any experiment which I can imagine; and therefore it is a case which serves well to discriminate between the two wholly distinct objections which I have attempted to establish against this application of the theory of probabilities to the distribution of the stars. The *first* objection (Art. 20) would apply to the results of that experiment treated by Mitchell's method (as has been explained in its proper place). The *second* objection (Art. 28) does not apply, because the action of the sieve is not "random scattering" in the extreme sense in which our utter ignorance of causation would require it to be represented. It is only *random* in respect of subordinate distribution, the mechanical arrangement is such as to produce a clear and well-

defined tendency to ultimate uniformity. The large dimensions of the sieve, the symmetry of its pattern, and its position vertically above the chess-board, are the conditions of this approximate uniformity of distribution, and they are evidently far from casual circumstances. To generalize the experiment with the sieve, we must imagine a great number of sieves pierced with holes in every conceivable variety of pattern, one of which has been selected at random for a given experiment. By these considerations I believe that the ordinary axioms of probability are not interfered with, but that the problem of the distribution of stars "by chance as it may happen" receives as definite an illustration as so vague an assumption admits of, and is shown to be incompatible with the axiom that "each star is as likely [not hypothetically, but actually] to be in one situation as another." This would only be true on the almost infinitely improbable supposition of a set of dice, not so formed expressly, being each and all perfectly homogeneous and unbiassed.

38. On the whole, the conclusions of this paper may be thus summed up*.

(1.) The fundamental principle of Mitchell is erroneous. The probability expressed by it is an altogether different probability from what he asserts. His calculations are also apparently inaccurate, in some instances at least†.

(2.) All the *numerical* deductions of his successors are equally baseless.

(3.) Were Mitchell's principle just, a perfectly uniform and symmetrical disposition of the stars over the sky would (if possible) be that which could alone afford no evidence of causation, or any interference with the laws of "random ;"—a result palpably absurd.

(4.) Special collocations, whether (*a*) distinguished by their symmetry, or (*β*) distinguished by an excessive crowding together of stars, or the reverse, inevitably force on the reasoning mind a more or less vague impression of causation;—an impression necessarily vague, having nothing absolute, but depending on the previous knowledge and habits of thought of the individual, therefore incapable of being made the subject of exact [*i. e.* mathematical] reasoning.

(5.) The form of error into which those have been led who had stated numerical probabilities against given arrangements of stars being the result of accident, is two-fold. *First*, a con-

* These conclusions, excepting the last, are copied *verbatim* from the paper read to the Physical Section of the British Association in August 1850.

† It would probably be more correct to say that all Mitchell's calculations are wrongly deduced from his own premisses.

fusion between the *expectation* of a given event in the mind of a person speculating about its occurrence, and an *inherent improbability* of an event happening in one particular way when there are many ways equally possible. *Secondly*, a too limited and arbitrary conception of the utterly vague premiss of stars being "scattered by mere chance, as it might happen;"—a statement void of any condition whatever.

39. Having thus stated broadly the errors of deductive reasoning (as I conceive them to be) to which the sanction of many eminent authorities has been given, I would guard myself, once more, in conclusion, from being supposed to doubt either the fact of the physical connexion of double stars generally, or to deny that Mitchell was right in *suspecting* that fact from the mere circumstance of the proximity of two stars observed in so many cases. This was, on his part, no more than the first stage of an induction (the nature of which is now so well understood by all students of physical science), being a mere collecting and arranging of a certain class of facts, and assuming the probability of a common cause to be afterwards investigated. Mitchell's appeal to the mathematical theory of probabilities was a false step, tending to weaken not confirm his argument. Still less do I object to the inference drawn by some of Mitchell's distinguished successors in favour of the physical connexion of binary stars from the following fact, viz. that if we arrange such stars in groups, each of which includes double stars whose angular distance is between certain limits, as for instance from $0''$ to $4''$, $4''$ to $8''$, $8''$ to $16''$, &c., the class of close stars is actually far more numerous than those contained within wider limits, and which we might therefore fancy ought to be of more frequent occurrence. This is a very curious and a very striking fact, and carries the process of induction one step higher than it was carried by Mitchell. It shows a greater frequency of occurrence for small distances, and furnishes something like a law connecting those two things, the fact of duplication and the angular distance. Such an empirical law, if well supported by facts, is undoubtedly a good inductive argument. We are brought nearer to the conception of a cause by this observation, and our conviction is complete when we observe one such star revolving round another with a clear orbital relation.

40. I should hardly have thought it necessary to state so much, had not my views been strangely misunderstood, particularly by an ingenious writer in the *Edinburgh Review**,

* *Edinburgh Review*, July 1850.

who has represented me as making averments altogether foreign to my thoughts, and has quite overlooked the precise objections contained in my letter printed in the *Philosophical Magazine*, and the precise statements quoted from Herschel's *Astronomy*, which I have again cited in § 5 of this paper. The reviewer charges me with "a singular misconception of the true incidence of the argument from probability." I can only say that my error, if such it be, is one sanctioned by Sir John Herschel, to whom I referred as my authority; for in his "Outlines" he states Mitchell's argument from the Pleiades, and Struve's arguments against the chance combination of a solitary double, or treble star, as I have shown in the text, but makes no mention, so far as I have been able to discover, of a generalized argument from the number of double stars in the different classes, depending upon the disproportionate number of *close* double stars to those wider apart; and either to understand or express that argument aright, it would be an insult to common sense to invoke the aid of the theory of probabilities; being neither more nor less than this, that the occurrence of double stars increases exceedingly much slower than the area of space including two members of the same pair. I never found fault with this argument: it is an argument good as an induction, but nothing more; the argument complained of by me was this,—"that the odds are 9570 to 1 against any two stars falling within 4" of each other, and the odds against any two such stars fortuitously falling within 32" of a third is not less than 173,524 to 1;" and Sir John Herschel's inference from these premisses, and these alone, "the conclusion of a physical connexion of some kind or other is THEREFORE unavoidable." The reviewer ignores these statements, joins issue on a point never before brought into the controversy, and silences objection by the general assertion that if this case be given up "there remains no possibility of applying the theory of probabilities to any registered fact whatever,"—a conclusion which of course I dispute as simply an assumption of the point in debate.

41. I am sorry to have to allude even thus briefly to the controversial article in the *Edinburgh Review*; but it has been brought under my notice in a manner so authoritative that I could not overlook it; although, as an argument, it appears to me to be destitute of weight, and in fact to proceed on a misapprehension. To criticize it minutely would not only prolong this paper beyond due limits, but would involve me in the invidious, and by me unsought task of dwelling on other mistakes of eminent authors connected with the subject of these pages. The principle of Mitchell is, evidently, that

sanctioned and adopted by all succeeding writers on this particular application of the doctrine of chances*. I have purposely avoided citing the names of many of these writers, and the passages in their works where Mitchell's results are incorporated, and his method of research applauded, although I could have made a list both numerous and distinguished. It was necessary for me however to show how far that principle was pushed by living writers of eminence, thus becoming more widely spread and perpetuated. The necessity of such a definite citation has become evident from the misapprehensions to which I have been exposed. If Mitchell's deductions had remained buried in the heavy quartos of the Philosophical Transactions, I should not have thought that the refutation of them was so important.

Phesdo, Kincardineshire,
September 19, 1850.

Note A.

Admitting for the moment that Mitchell is correct in assigning the probability $\left(\frac{13130}{13131}\right)^n$ for *not* finding one out of n stars within a distance of 1° of a given star, I conceive that he is wrong in raising that quantity *again* to the n th power in order to include all the equal chances which exist in favour of B, C, D being duplicated equally with the given star A. For though the probability of a compound event is measured by the product of the several probabilities of the *independent* events which must concur to produce it, the several probabilities with reference to the different stars cannot be considered as altogether independent. The duplication of A by B, and of B by A, which constitute one event, are counted separately; and again, the probability of the duplication of the star A by the star B, and that of the star C by the star B (which are two independent events), are not independent of the duplication of C by A.

* The Edinburgh Reviewer thus explicitly adopts Mitchell's principle of reasoning refuted by our "First Objection." "As probability is the numerical measure of our expectation that an event will happen, so it is also that of our belief that it has happened, or that any proposed proposition is true. Expectation is merely a belief in the future, and differs in no way so far as the measure of its degree is concerned from that in the past."—*Ed. Rev.* July 1850, p. 7. I do not of course mean to say that *all* writers on Probability have adopted Mitchell's argument, although none that I know of condemn it. I must however cite Mr. Leslie Ellis's paper in the Cambridge Transactions (vol. viii. part 1), as containing a bold and clear exposure of the errors sanctioned by many eminent philosophers on questions of chance, —errors a good deal allied to that which we have here discussed.

The case may be illustrated by a reference to one of those games of chance which serve best to fix the ideas as to every kind of hazard, adopting of course Mitchell's opinions on the mode of conceiving the fundamental question. Let there be as many dice as stars, and let n be that number. Let each die have p sides, numbered 1, 2, 3, &c. I say that the chance of doublets occurring amongst the n dice thrown at once is the same as that of two stars being found at a less distance than the radius of a small circle of the sphere which includes an area $\frac{1}{p}$ th of the entire surface of the sphere. For take any star, and describe round it as a centre the small circle which includes $\frac{1}{p}$ th of the spherical surface, a second star falling within this circle will constitute it a double star; and the chance against its doing so is evidently p to 1, or the chance of not throwing doublets with two dice having p sides. But there are n stars altogether, and each may duplicate the other under like conditions, so that the probability of the occurrence of a doublet amongst n dice is the same as the chance that two of n stars shall duplicate one another, or shall be included in a circle 1° in diameter; the occurrence of a triplet represents three stars being within distance of one another, and so forth.

Having myself no aptitude or practice in calculating chances, and knowing the oversights sometimes made in such cases even by persons who have been considered as authorities on the subject, I requested a mathematical friend, whose skill in these matters gives the utmost attainable assurance of his accuracy, to solve this and some other questions to which I shall have occasion to refer. He kindly complied with my request, and has given me the following solution.

The number of arrangements which the n dice can form *without* duplication is the same as the number of permutations of p things taken n together, or

$$p(p-1)(p-2) \dots (p-n+1);$$

also the total number of falls is p^n . Consequently the chance of a fall *without* duplication is

$$\frac{p(p-1) \dots (p-n+1)}{p^n}.$$

And the chance that two or more of the dice show the same face is

$$1 - \frac{p(p-1) \dots (p-n+1)}{p^n},$$

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which may be written

$$1 - \frac{1.2 \dots p}{1.2 \dots (p-n)} \cdot \frac{1}{p^n}.$$

We may apply this theorem to Mitchell's case of the star β Capricorni, which consists (he states) of two stars within $3\frac{1}{3}$ minutes of one another, and there are in the sky 230 stars of similar brightness. Now the area of a space $3\frac{1}{3}'$ radius is 4254603 less than the area of the entire sphere. Hence $p=4254603$ and $n=230$. The value of the above expression, carefully calculated by the aid of Stirling's theorem*, gives the chance for the proximity in question = .00617, or about $\frac{1}{160}$. Mitchell finds it $\frac{1}{81}$ (Phil. Trans. lvii. p. 246). The procedure of Mitchell where *several* stars are concerned—for instance the Pleiades—seems to me to be still more deficient in evidence; and, in fact, I find from my mathematical friend, that when the question is as to the chance of *several* such dice showing at once the same face, the problem rises to an excessive degree of complication. I conclude, therefore, that the probability of 500,000 to 1 against the fortuitous concurrence of the Pleiades is, even granting all the premisses, erroneously calculated.

Note B.

In the *first* supposed case, the probability of the compound event of 100 dice turning up all the running numbers 1 ... 100 may be conceived to be the product of the independent probabilities of 98 out of the 100 dice showing the numbers 1 ... 98, or any other different numbers which are predetermined (we will call the probability of this $\frac{1}{P}$), and of the two remaining dice (having 100 faces each) showing the two faces 99 and 100, or the two remaining numbers. As this latter event may happen two ways, the probability of it is

$$2 \times \frac{1}{100} \times \frac{1}{100} = \frac{1}{5000}:$$

and $\frac{1}{5000} \cdot \frac{1}{P}$ is the probability of the given compound event. In the second supposed case, 98 numbers are to be turned up as before, with a probability $\frac{1}{P}$. The two remaining ones are

* The probability is
$$1 - \frac{1}{e} \left(\frac{p}{p-n} \right)^{p-n+\frac{1}{2}}$$

When n is much greater than 1, and p than n , this is almost equal to $\frac{n^2}{2p}$.

to be both 99. But this event can happen but one way; that is, by each die showing the number 99. Consequently the probability for this second event is

$$\frac{1}{100} \times \frac{1}{100} \times \frac{1}{P};$$

or but half that for the former.

The same result is obtained from the general formula of Note A, applied to find the chances of *no* duplication and of *one* duplication when $n=p=100$.

POSTSCRIPT.

In finally sending this paper to the press, after several modifications and additions, I may be permitted to express a hope that its arguments may be weighed with deliberation, and that first impressions, however strong, of a contrary tendency may be distrusted. The extreme generality of the considerations which enter into the discussion renders it difficult to lay down propositions sufficiently large to include them, and yet free from the risk of error through that very generality. I would therefore invite attention to the particular conclusions which I negative, and which must be maintained, I conceive, by any one who is disposed to adopt the views of Mitchell and his followers:—(1.) That there is any *calculable* probability, such as 9570 to 1, against the observed occurrence of 2 stars out of more than 10,000, within 4" of one another, having been fortuitous (Arts. 5, 40). (2.) That the fact of two stars being seen within an infinitely small distance of each other, amounts to a mathematical proof of the *certainly* of their being physically connected (Art. 30). (3.) That were the stars uniformly spaced over the heavens or arranged with perfect symmetry, no argument could be alleged against such arrangement being the result of chance, but any deviation from symmetry would raise such an argument (Arts. 30, 31, 32).

I have now devoted a great deal of time and attention to the consideration of this subject. I have profited too by the kind criticisms of several friends, to whom I submitted the argument. I do not think that my opinions are likely to change; and other engagements now require all my attention. Should this paper call forth any controversial remarks, I hope I may be excused (for the present at least) from entering further into the discussion without the imputation of want of courtesy. Silence, instead of implying consent, ought in such a case to signify the reverse; for, should I be satisfied that I am wrong in any point, I should in that case feel it a duty to acknowledge it.

Edinburgh, 16th November, 1850.

LV. *Description of a new Electrical Machine.*
 By W. H. BARLOW, Esq., F.R.S., M. Inst. C.E.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

Derby, April 9, 1850.

THE following short description of a new electrical machine may not be uninteresting to some of your readers.

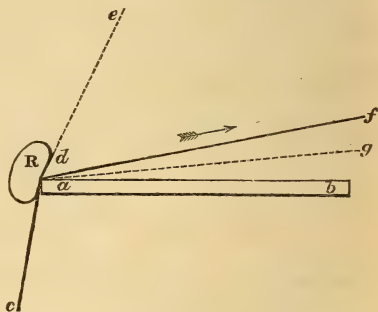
The highly electric properties of gutta percha have been commented upon by Dr. Faraday; but I believe it is not generally known that this material affords the means of producing in a very simple manner an amount of electricity as great as that of the common electrical machine.

There is a description of gutta percha sold which is thinner than common paper, about three feet wide, and in long lengths. If a sheet of this substance, of about four or five feet superficial area, be laid on a surface or held against the wall of a room and rubbed with the hand or a silk handkerchief, and then carefully removed by the extreme edges and held suspended in the air, it will give off a brush-like spark of several inches in length to the knob of any conducting surface presented to it.

A similar effect may be produced by causing the sheet of gutta percha to be passed once over one, or between two rubbing surfaces; but in order to obtain the best effect, certain conditions appear necessary, which I will here only describe as I have found them, without occupying space in your valuable *Journal* to enter into the cause of the effects produced.

If ab be any surface, such as that of a common table, and cdf be a sheet of gutta percha made to pass over it in the direction exhibited by the arrow, so as to produce friction at d , then the best effect is exhibited when df forms an angle of about 10° with the surface ab : if it be brought to a smaller angle as dg , or a larger one as de , the amount of

Fig. 1.

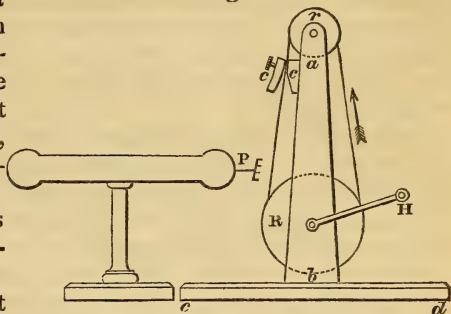


electricity excited is not so great; but a still greater effect is produced if another rubber be applied outside the gutta percha at d ; and if the surface of ab and the cushion R be of silk or horsehair, and the pressure and speed of rubbing moderate, the amount of electricity excited is very considerable.

Having found these effects to be constant, it occurred to me

that they might be employed in producing an electrical machine at once cheap and simple; and I have accordingly had a machine constructed as follows:—*abcd* is a wooden frame which carries two wooden rollers, *R* and *r*. The lower roller, *R*, is about six inches diameter, and a handle, *H*, is attached to the axle.

Fig. 2.



The upper roller is about three inches diameter.

A band of thin sheet gutta percha about four inches wide is made to pass round the rollers, fitting them very tightly. *CC* are two cushions covered with silk, and connected together so as to press the gutta percha at their upper extremities, and opening towards their lower extremities at an angle of about 20° . When the handle of the machine is turned, causing the gutta percha band to pass at a moderate velocity in the direction denoted by the arrow, electricity is given off at *P*, about three or four inches below the cushions; and if a conductor be applied, as shown in the sketch, the apparatus may be used as a common electrical machine.

The quantity of electricity developed increases with the surface of gutta percha; and from the energy exhibited by so small a band, it is evident that considerable power might be obtained by a more extended application of this material.

I observed that gutta percha may be excited both positively and negatively.

If a strip about two feet long and two inches wide be laid on a surface and rubbed, the two extremities when suspended in the air repel each other, and the electricity developed is that termed “resinous.” But if the strip of gutta percha be folded double and rubbed, the upper side exhibits “resinous,” and the lower side “vitreous” electricity, and the two extremities attract each other.

I am, Gentlemen,

Your obedient Servant,

W. H. BARLOW.

P.S. Since writing the above, John Westmoreland, one of the workmen in the establishment of Mr. Davis, optician, at

Derby (where the apparatus above described was made), has constructed a gutta percha electrical machine, similar in principle, but with several improvements.

In this machine a thicker description of gutta percha band is employed. The upper and lower rollers are of equal diameter; and the rubbers, which are brushes of bristles, four in number, are placed outside the band and opposite to the axis of each roller.

A double conductor connected by a curved brass rod passing over the top of the machine is applied, similar in form to the conductor of the plate-glass machines; and there is an ingenious tightening apparatus, to correct the expansion and contraction of the gutta percha band.

The machine is exceedingly handsome in appearance; and the employment of the thicker gutta percha removes the difficulty experienced in the former apparatus from the band becoming folded.

The band is about four inches wide; and the electricity given off appears to be of higher intensity, and, under favourable states of the weather, nearly as much in quantity as that of an ordinary plate-glass machine.

The improvements above described are all due to John Westmoreland; and this machine (which is beautifully finished, and is, I believe, intended for the Exhibition of 1851) is the result of his labours after the usual hours of work in Mr. Davis's establishment.

W. H. B.

Derby, Nov. 14, 1850.

LVI. *Notice of the occurrence of Chalk Flints and Greensand Fossils in Aberdeenshire.* By WILLIAM FERGUSON, Esq*.

MANY years have elapsed since it was first noticed that quantities of flints containing organic remains characteristic of the chalk, as well as certain organic remains belonging to the greensand formation, existed in Aberdeenshire. Dr. Knight, of Mareschal College, Aberdeen, was aware of the fact, and through him it was communicated to Dr. Thomas Thomson of Glasgow College nearly twenty years ago. Mr. Christie of Banff published a notice upon the occurrence of flints at Boyndie Bay, Banffshire, in the *Edinburgh Philosophical Magazine* for 1841; and a paper on the same subject, of which the present is a condensed view, was read before the *Philosophical Society of Glasgow*, Session 1848-1849. Since

* Communicated by the Author.

then the attention of the British Association has been drawn to them at their last meeting in Edinburgh by the Rev. J. Longmuir of Aberdeen, who had previously, in 1847, published a notice on them in the *Witness* newspaper. Still the knowledge of these curious deposits is confined to a few; and it is hoped the following short description of them will not be unacceptable or uninteresting to English readers.

On glancing at the geological map of Scotland, it will be observed that the portion of Aberdeen and Banff shires cut off by a line drawn from the mouth of the Ythan to that of the Doveron, is, with the exception of a narrow band of old red sandstone, represented by the colours denoting granite, gneiss, mica-slate and quartz rock. These rocks come to the surface everywhere, and yet superimposed on these, lie immense multitudes of chalk flints, and at one place at least a small patch of greensand.

Throughout the whole of this district, with the exception of Mormond, which is 810 feet high, there are no hills. Still the country is by no means level; and it presents to the eye in the ever-recurring hillocks and deep gullies, together with large accumulations of rolled gravel, ample evidence of widely extended denudation.

Running slightly to the south of west, there is a ridge of high ground taking its rise nearly at Buchanness, and stretching across the country continuously for eight or ten miles. At its eastern extremity it branches out. One of the branches terminates south of Buchanness in the granitic mass of Stirling Hill; the other ridge runs north of Buchanness, and may be said to terminate in the granitic escarpment of the Black Hills. All along the shore between these points, wherever the rocks admit of a beach, quantities of water-worn flints are found mingled with the other pebbles, evidently brought there by the waves. They are also found, although sparingly, on the southern ridge, or Stirling Hill. On the Black Hill and neighbouring hill of Invernettie, the surface is almost covered with them. This ridge, at the distance of about seven and a half miles from the sea at Salthouse Head, attains an inland distance of about five miles from the coast opposite Slains. The flints are met with on the surface at several points along this line. The ridge is bare and moorish, covered with peat and heather, and this prevents the accurate tracing of the flints. At this point, however, seven and a half miles along the line of the ridge, and about five miles from the sea, opposite Slains, they have been laid bare.

They occur at the extreme verge of the parish of Old Deer, and are principally seen upon the farm of Bogingarry, on the

estate of Kimmundy. The ridge of hill on which they occur here trends to the north, coming round again towards the west, so as to expose to the south a bay-like hollow. The hill is covered with moss and heather, part of which has been planted. The south face of the hill has, however, been under cultivation during the last twenty-five or thirty years. The flints are seen on the surface, commencing pretty far up on the east side of the hollow, and following at the same height the form of the bay, disappearing among the heather, which has not yet been removed on the extreme west. They are in great abundance, covering a space of some twelve to twenty yards in breadth.

About 1830, in cutting a ditch to carry off the surface-water from the garden of the farmhouse of Bogingarry, the bed of flints was come upon, and found to be of considerable thickness. The ditch ran from south-west to north-east, traversing the flint bed, and a short cross one lay in the line of the bed.

When I saw the ditch first, about 1839, it had become partly filled up and had a most singular appearance. It was crossed by the road to the house, and the water-way of the bridge was quite choked with rounded flints of all sizes. Above the bridge, the bottom of the ditch was quite covered with rounded flints brought down by torrents. As you ascended the burn you could see the nature of the ground. The layer of soil was extremely thin, and below it the ditch was cut through a stiff yellow clayey gravel, so hard as to be pierced with extreme difficulty. Until you reach the bed itself, very few flints are to be seen among the clay. The top end of the ditch and the cross one are in the bed. The flints lie closely packed together, imbedded in the already mentioned clayey matrix. They contain numerous organic remains.

Near Peterhead there have been found considerable varieties of the *Echini* family, occasionally entire, but more generally only small portions of the impressions of these shells are found. Single spines frequently occur, and are distinctly marked. The *Inoceramus*, *Pecten* and *Terebratula*, are very abundant.

Flints are also found on the surface, on the adjoining hill of Skelmuir. South-westward they are found again on the hill of Dudwick in the parish of Ellon. This seems to be their southernmost limit. In some of the localities they are shivered to atoms, retaining their sharp fracture; and this so universally in these places, that an unbroken one is scarcely to be seen.

Mr. Christie of Banff, in the paper already referred to, states that they occur at Boyndie Bay in that shire; and also in a mass of diluvium covering the high grounds between

Turriff and Delgaty Castle. The flints at Boyndie Bay are found strewn along the shore, and contain traces of zoophytic organic remains. Those at Delgaty are likewise characterized by similar remains. The station at the latter place is ten miles from the sea, and is the highest ground in the neighbourhood. The flints are found in the diluvium cresting the hills.

In my collection of fossils from Bogingarry and adjoining localities, there are impressions of portions of spines, and also casts of at least three varieties of the *Echinus*. There are also casts of *Belemnites*, *Inoceramus*, *Terebratula*, *Pecten*, *Plagiostoma*, *Turbinolia*, *Flustra*, &c., together with other remains not easily made out. From the remains in the flint, existing for the most part as casts and impressions, it is difficult, indeed it is almost impossible, to make out many of them with sufficient certainty to name them. I have, however, met with instances in which the shell is actually preserved.

The greensand is found at Moreseat in the parish of Cruden. This locality is on the ridge of high ground already mentioned as running south-west from Buchanness. In the immediate neighbourhood, the evidence of extensive denudation is very marked. Attention was called to the deposit in question here quite recently. The proprietor of the farm, Mr. Johnstone, had built a new mill; and in making an excavation for the water-wheel, several large and curious shells imbedded in a fine-grained compact sandstone were turned up. The sandstone itself, previously unknown in this locality, would have been enough to attract notice—far more the shells. One of these was the fragment of a cast of an *Ammonite*. The portion preserved was at least four inches in diameter. The deposit was about nine feet below the surface of the ground, and yielded water freely. The wheel is now built in, and the locality in consequence cannot be examined. Several hundred cartloads of material were taken from this excavation, and used to fill up inequalities in the surrounding fields. It is to be regretted that this was not examined previous to being deposited, as it is now beyond reach. About 400 yards to the north-east of this point the shells were again met with, in opening a ditch round a field newly reclaimed from moss. I had this year (1850) an opportunity of examining it here. It is from one to three feet below the surface, and is traceable in the ditch one or two hundred yards. An excavation of about seven feet in depth was made, and the section presented irregular layers of unctuous clay of a dark brown colour and soapy feel; and so tough and adhesive, as to render it a work of considerable labour to dig it out. Interstratified with this clay were thin layers of a compact sandstone. The layers of

sandstone were not continuous; they graduated into each other, thinned out, disappeared and reappeared most confusedly. They were very much inclined, dipping towards the south. The whole mass had much the appearance of having been drifted; although from the nature of the material, and the state of preservation in which the shells are found, it does not appear as if it could have been transported far. The sandstone is tough and soft when newly dug, but hardens on exposure to air, and becomes light-coloured in drying. When wet it presents a mottled appearance, the colouring being greenish; when dry, this almost disappears. The exterior surfaces are quite reddened with iron. Many of the remains are casts, and are on the outer surfaces of the fragments of sandstone as well as in the heart of the masses. Of the remains themselves, flattened Spatangi are most abundant. Several species of Ammonites and Belemnites, as also of Cardium, Terebratula, Trochus, Solarium, Cerithium, &c., are met with. In a few rare instances the shells themselves are preserved.

The following theoretical remarks are added with much diffidence, nearly as they were originally delivered to the Glasgow Society.

From our brief survey of the surrounding country, we saw that the prevailing rocks are the crystalline and stratified-unfossiliferous. Only in one instance does a limestone occur with organics (an Ammonite)(?), which might consequently belong to the secondary group. Old red sandstone occurs at Aberdeen, and again (certainly) at Gamrie, but it has not been positively seen at any point between, although it has been supposed that it may nevertheless envelope the older rocks along the coast beneath the sea-level. Oolite and Wealden occur in the neighbourhood of Elgin. The distance between these beds at Llanbride and the flints at Buchan cannot be less than between fifty and sixty miles. Water-worn fossils of the lias occur at Blackpots near Banff, but there they are manifestly in a diluvial clay. The old red sandstone is the newest rock that is known to occur over all Banffshire; consequently the whole of this county comes between the deposit under consideration, and the newer formations of Morayshire.

This greensand of Cruden is the only rock at all approaching, in the geological sequence, the chalk beds from which the flint boulders must have been derived. We are forced to conclude concerning it, that if it is not *in situ*, it is at least not far removed from it.

The question then arises, How came the flints and greensand there, and whence?

Mr. Hugh Miller, cautioning the young geologist against

concluding that because he finds a rock resting upon gneiss it is therefore low in the geological scale, instances as an example of the error such a conclusion would lead to, the flints and chalk fossils of Banff and Aberdeen lying immediately over it in these counties, and adds, "it is probable that the denuded members of the cretaceous group once rested upon it there." Prof. Jameson, in the *Edinburgh Philosophical Magazine*, states the same opinion, adding, that it will probably be found in some of the hollows of this part of Scotland.

This is one theory,—that the lower beds have been wanting here, and the chalk of this bed has been removed by denudation, leaving the flints resting on the granite. Opposed to this theory is the fact, that the flints are invariably very much water-worn. True, even according to it they would have presented such an appearance, but not necessarily to such an extent; and it seems that a denuding agency sufficiently powerful to produce the rolled effect noted would have removed them as well as the other beds, especially as they occur, *not in hollows*, but almost always on the sides and near the summits of hills. Mr. Nicol states his opinion thus:—"Probably these recent secondary formations once existed here, or may still be covered by the sea, and connected with the similar beds in the Moray Frith. This opinion is confirmed by the occurrence of lias, containing coal, at Hogenaes in the south of Sweden, where it rests on gneiss and is covered by chalk." This leads on to another theory which has been suggested to account for these flints; viz. that however such secondary beds may have once existed here, these individual water-worn flints owe their origin to a transporting agency which has brought them from the chalk formations of the northern continent.

The volcanic and tidal agencies (the latter modified by local currents) seem to assume a direction between south-west and north-east. All the mountain ranges and great formations of our island assume in general that direction. The great mountain range of Norway assumes the same. I am too unacquainted with Norwegian geology to be able skilfully to connect it with Scottish. At Christiania there is a group belonging partly to the lower and partly to the upper Silurian rocks. True chalk with flints has been clearly determined in some parts of Denmark. This Danish group may have been continued into Norway at one period, and afterwards removed by denudation, the same agency transporting the flint nodules to our shores.

It may bear against such a supposition of transportation, that the direction of the currents seem usually to have been from south-west to north-east, and that for this theory they require

to have been reversed. It may be suggested, Might not the elevation of the great mountain ranges of the Continent have been sufficient to cause a current from its shores capable of exercising the transporting power required? The presumption is, however, against such a supposition.

Standing on the ridge of the Hill of Kinmundy, and looking towards the south and east, there is spread out before the eye a wide expanse. Slightly to the north of eastward the ridge is continuous to the sea at Buchanness; westward it undulates, receding northward, and again stretching out a promontory to the south. Beyond this there is a valley, and again the hills rise, stretching away westward and northward, running out in a series of high grounds by Dudwick towards Turriff and Delgaty, and so onwards to the sea at Boyndie. Between this ridge and the sea, on the south and south-east, there stretches out from the sort of bay described a breadth of five or six miles of country, presenting inequalities of surface, but in the main level till it reaches the sea, with a coast line elevated 180 to 200 feet above water-mark. Over this valley calcareous sands occur, and near its centre is the greensand formation. And standing, as I have said, on the hill ridge, and marking, as one cannot fail to mark, the band of flint boulders that line near their highest and at an equal elevation the various bays and promontories, it requires no great stretch of imagination to conceive of the waves of the German Ocean as having once rolled even hither, bearing with them, and depositing on their innermost bounds, the rounded flints that now mark their ancient shore.

I have already stated, that the shores of the little bays near Peterhead present large quantities of the rounded flints. These may be either brought down by streams, or cast up from the sea, or both. I have also inferred, from the condition of my specimens of organic remains from the Cruden greensand, that that formation, if not *in situ*, cannot at least be far removed from its original position; not presenting evidence of being water-rolled, and not capable of undergoing, without destruction, that process.

I wish to connect these two facts with an idea hinted at by Mr. Nicol, and additional grounds for which have been pointed out to me by Mr. Hugh Miller. Across the southern district of England we have a certain sequence of geological formations, including in regular succession the Lias, Oolite and Wealden, succeeded by the Cretaceous. Across that portion of Scotland immediately to the north of the district now under consideration, we have part of the same sequence commencing with the lias. This formation at Cromarty, and at Brora in

Sutherlandshire, is considerably to the west of the first appearance of the same formation in England: but this results naturally from what was before mentioned of the geological formations running, not east and west, but north-east and south-west; not right, but diagonally across the country. We have thus lias at Cromarty, and a lower oolite near Elgin. May it not be possible, that all we want to complete the remaining members of the series is simply to be able to carry out our section into the Moray Frith?

Such an hypothesis receives confirmation from the fact, that in the neighbourhood of Elgin are beds containing Wealden fossils, "which," says Mr. Nicol, "we are led to suspect are not original formations, but fragments of more extensive beds, perhaps drifted to this place." The diluvial clay containing lias fossils at Blackpots may also indicate a formation beneath the waters of the bay. By referring to the geological map of England, it will be seen that the greensand accompanies the chalk lying on the west of it and on the east of the lias, to the shore of the Channel. Our patch of it at Cruden might form part of the termination of a similar stripe, unless it, too, can be accounted for in the same way as the Moray Wealdens, by supposing it a drifted fragment from the north.

May we then fairly infer, that at one period the space now occupied by the Moray Frith contained a perfect sequence of the secondary formations; that first the soft chalk strata suffered denudation by the ordinary action of north-easterly gales, currents and drift-ice, and that the roll of the German Ocean piled up its debris in the shape of these water-worn flint boulders along its successive ancient shores; and that the Wealden and oolite of Elgin, and lias of Blackpots, followed in the same course?

That part of this theory applicable to the lias of Blackpots Mr. Miller states thus, in his description of that deposit:—"There had probably existed to the west or north-west of the deposit, perhaps in the middle of the open bay formed by the promontory on which it rests—for the small proportion of other than liassic materials which it contains serves to show that it could be derived from no great distance—an outlier of the lower lias. The icebergs of the cold glacial period, propelled along the submerged land by some arctic current, or caught up by the gulf-stream, gradually grated it down, as a mason's labourer grates down the surface of the sandstone slab he is engaged in polishing; and the comminuted debris, borne eastward by the current, was cast down here."

At Blackpots the lias fossils occur in clay containing few

other boulders. At Boyndie, further west, the flint boulders cover the shore; and at Delgaty, ten miles inland, they occur in great abundance, along with boulders of quartz rock, but no fossils except their own. It would therefore appear, that we owe the flint boulders and the lias boulders to different periods; and as the chalk overlies the lias, it may be that its denudation was completed, and its fossils thrown upon the high grounds of the interior, previous to the formation of the boulder clay containing the fossils of the lias. Although apparently not here, the boulder clay has in other places (as on the banks of the Thorsa in Caithness) been found to contain "fragments of chalk flints, and also a characteristic conglomerate of the oolite," as well as comminuted fragments of existing shells. (H. Miller.) These facts seem also to favour this hypothesis.

The subject altogether is one involved in considerable darkness, and it is perhaps vain to attempt any generalization upon it till the local geology has been far more accurately examined and determined.

LVII. *On a Porismatic Property of two Conics having with one another a contact of the Third Order.* By J. J. SYLVESTER, M.A., F.R.S.*

IF two conics have with one another a contact of the third order, *i. e.* if they intersect in four consecutive points, it will easily be seen that their *characteristics* referred to coordinate axes in the plane containing them must be of the relative forms $x^2 + yz$, $k(y^2 + x^2 + yz)$ respectively, y characterizing their common tangent at the point of contact†.

Hence if we take planes of reference in space, and call t the characteristic of the plane of the conics, the equations to any two conoids drawn through them respectively will be of the relative forms

$$U = x^2 + yz + tu = 0$$

$$V = y^2 + x^2 + yz + tv = 0.$$

* Communicated by the Author.

† These relative or conjugate forms are taken from a table which I shall publish in a future Number of this Magazine, exhibiting the conjugate characteristics in their simplest forms, correspondent to all the various species of contacts possible between lines and surfaces of the second degree. This table is as important to the geometer as the fundamental trigonometrical formulæ to the analyst, or the multiplication table to the arithmetician; and it is surprising that no one has hitherto thought of constructing such.

Using W to denote $V-U$, and (W) to denote what W becomes when ey is substituted for t , we see that W and (W) are of the respective forms $y^2 + tw$ and $y\theta$; showing that the former is the characteristic of a cone which will be cut by any plane $t-ey$ drawn through the line (t, y) in a pair of right lines; or, in other words, that one of the cones containing the intersection of the two variable conoids (V and U) will have its vertex in the *invariable line* which is the common tangent to the two fixed conics: this proves the theorem stated by me hypothetically in a foot note in one of my papers in the last Number of the Magazine. The steps of the geometrical proof there hinted at are as follows.

The four consecutive points in which the two conics intersect will be consecutive points in the curve of intersection of the two variable conoids. This curve lies in each of four cones of the second degree. Every double tangent plane to it passes through the vertex of one amongst these. The plane containing four, *i. e.* two (consecutive) pairs of consecutive points, is a double tangent plane, and will therefore pass through a vertex; but four consecutive points of a curve of the fourth order described upon a cone, and lying in one tangent plane thereto, can only be *conceived* generally as disposed in the form of an $\int$, of which the belly part will point to the vertex; or, in other words, at any point where two consecutive osculating planes coincide so that the *spherical* curvature vanishes, the linear curvature will also vanish, *i. e.* there will be a point of inflexion at which, of course, the tangent line must pass through the vertex of the cone. This is the assumption felt to be true, but stated by me hypothetically in the paper referred to, because a ready demonstration did not at the moment occur to me. The legitimacy of this inference is now vindicated by the above analytical demonstration.

The methods of general and correlative coordinates and of determinants combined possess a perfectly irresistible force (to which I can only compare that of the steam-hammer in the physical world) for bringing under the grasp of intuitive perception the most complicated and refractory forms of geometrical truth.

26 Lincoln's-Inn-Fields,
October 30, 1850.

LVIII. *On the Rotation of a Rigid Body about a Fixed Point.*

By J. J. SYLVESTER, M.A., F.R.S.*

IN the Cambridge and Dublin Mathematical Journal for March 1848, an article by Professor Stokes, of the University of Cambridge, is ushered in with the words following:—

“The most general *instantaneous*† motion of a rigid body moveable in all directions about a fixed point consists in a motion of rotation about an axis passing through that point. This elementary proposition is sometimes assumed as self-evident, and sometimes deduced as the result of an analytical process. It ought hardly *perhaps* to be assumed, but it does not seem desirable to refer to a long algebraical process for the demonstration of a theorem so simple. Yet I am not aware of a geometrical proof anywhere published which might be referred to.”

The learned and ingenious professor is indubitably right, and might have trusted himself to assert less hesitatingly the necessity of demonstrating this proposition, which possesses none of the characters of a self-evident truth; but it is to be regretted that he should have stated it in such a form as naturally to lead the incautious reader to mistake the nature and grounds of its existence, which consist in this fact—that any kind of displacement of a body moveable about a fixed axis, whether instantaneous and infinitesimal, or secular and finite, is capable of being effected by a single rotation about a single axis.

The annexed simple proof of this capital law has the advantage of affording a rule for compounding into one any two (and therefore any number of) rotations given in direction, magnitude and *order of succession*. •

It will somewhat conduce to simplicity if we fix our attention upon a spherical surface rigidly connected with the rotating body, and having its centre at the fixed point thereof. When the positions of two points in this are given, the position of the body is completely determined.

Now evidently two points A, B may be brought respectively to A' B' (if $AB = A'B'$) by two rotations; the first taking place about a pole situated anywhere in the great circle bending at right angles AA', the second about A', the position into which it is brought by the first rotation. This view leads us to consider the effect of two rotations taking place successively about two axes fixed in *the rotating body*. Or again, we may make the plane A' B' revolve into the position AB round a pole

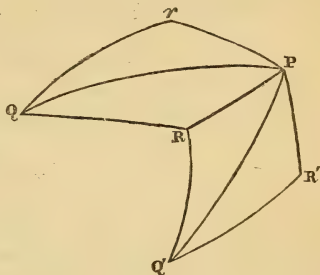
* Communicated by the Author.

† The italics do not exist in the original.

taken at the node in which the two planes intersect, and then the points A, B swing into their new positions A', B' by means of a rotation about the pole of the great circle, of which A' B' forms a part. This mode of effecting the displacement naturally suggests the consideration of the effect of rotations taking place successively about two axes fixed in space.

First, then, let us study the effect of the combination of a rotation (α) having P for its pole, followed by another (β), of which Q is the pole, P and Q being points in the surface of the revolving sphere.

In drawing the annexed figure, I have supposed that the two rotations are of the same kind, each tending, when a spectator is standing with his head to the respective poles and his feet to the centre, to make a point to his right-hand pass *in front of his face* towards his left-hand. Let now PQ revolve through $\frac{\alpha}{2}$ positively into the



position of PR, and through $\frac{\beta}{2}$ negatively into that of QR.

Then I say that the two impressed rotations α and β about P and Q will be equivalent to a single rotation about R, equal to twice the acute angle between QR, RP.

Let the first rotation about P bring Q to Q' and R to R'; it is clear that QPR, Q'PR, Q'PR' are all equal triangles. Therefore $R'Q'R = 2PQR = \beta$. Consequently the positive rotation β about Q' (the new position of R) will carry R' back again to R, its original position. Hence the actual motion which results from the successive rotations combined being consistent with R remaining at rest, must be equivalent to a single rotation about R.

To find its magnitude, let the second rotation carry P to P*; then the angular displacement PRP' (which is the required rotation of the whole body) is equal to twice the acute angle between Q'R, RP, which is the same as that between QR, RP, as was to be shown. Thus we see that the semi-rotations about three poles (considered as the angular points of a spherical triangle), which, taken in order, would bring the sphere back to its first undisturbed position, are equal to the included angles at such poles respectively.

* The reader is requested to fill in the point P' and join P'R.

If in our figure the order of the rotations had been reversed, PQr , QPr would have been taken respectively equal to PQR , QPR , but in the opposite side of PQ , and r would have been the resultant pole, the resultant rotation remaining in amount the same as before.

If either of the rotations had been negative, the resultant pole would be found in QR produced, viz. at the intersection of rQ or rP with PQ .

Calling the resultant rotation γ , we have always

$$\sin \frac{a}{2} : \sin \frac{\beta}{2} : \sin \frac{\gamma}{2} :: \sin QR : \sin PR : \sin PQ.$$

When the component rotations are infinitesimal in amount, R and r will come together in QP ; the order of succession of the rotations will be indifferent, and we shall have

$$a : \beta : \gamma :: \sin \frac{a}{2} : \sin \frac{\beta}{2} : \sin \frac{\gamma}{2} \\ :: \sin QR : \sin RP : \sin PQ,$$

which gives the rule for the parallelogrammatic composition of two simultaneously impressed rotations*.

If, next, we consider the effect of rotations about two poles, P and Q , fixed in space (supposing, as above, that they take place first about P and then about Q), we must take PQr equal to half the *contrary* of the rotation about P , and PQr to half the *direct* rotation about Q (the angle being now taken positive which was on the first supposition negative, and *vice versâ*); so that, retaining the original figure, the first rotation will bring r to R , and the second carry R back to r ; showing that r is the resultant pole, and that† $P'rP$, the resultant rotation, will be double the acute angle between Qr , rP , as in the former case.

To popular apprehension the important doctrine of uniaxial rotation may be made intelligible by the following mode of statement. Take a pocket-globe, open the case and roll about the sphere within it in any manner whatever; then closing the case, there will unavoidably remain two points on the terrestrial surface touching the same two points on the celestial surface as they were in apposition with before the sphere was so turned about in its case.

It is right to bear in mind that the whole of this doctrine is comprised in, and convertible with, the following easy geometrical proposition relative to arcs of great circles on any spherical surface, including the plane as an extreme case.

* Compare Mr. Airy's Tracts, art. "On Precession and Nutation."

† P' is not expressed in the figure given.

“The arcs joining the extremities (each with each in *either* order) of two other equal arcs, subtend equal angles at either of the points of intersection of two great circles bisecting at right angles the first-named connecting arcs*.”

The spherico-triangular mode of compounding rotations given in the above simple disquisition may easily be made the parent of a whole brood of geometrical consequences, which, however, I must leave to the ingenuity and care of those who have a turn for this kind of invention.

But I ought not to omit to invite attention to a remarkable form, which may be imparted to the theorems above stated for the composition of finite rotations, or rather to a theorem which may be derived from them by an obvious process of inference.

Let $P, Q, R \dots XZ$ be any number of points on a sphere capable of moving about its centre, joined together by arcs of great circles so as to form a spherical polygon. Imagine any number of rotations to take place about these points in succession as poles. It matters not which is considered the first pole of rotation, but the *order* of the circulation must be supposed given, as, for instance, $P, Q, R \dots XZ$, or $QR \dots XZP$, or $R \dots XZPQ$, &c. This will be one order; the reverse order would be $PXZ \dots RQ$, or $QPXZ \dots R$, &c.

I shall suppose the circulation to be of the kind first above written. Now we may make two hypotheses:—

1. That the poles are fixed in space.
2. That they are fixed in the rotating body.

In the first case, let the rotations about the given poles $P, Q, R, S \dots XZ$ be double the amounts which would serve to transport PQ to QR , QR to $RS \dots XZ$ to ZP respectively.

In the second case, let the rotations be double the amounts which would carry PZ to $ZX \dots SR$ to RQ , RQ to QP respectively. Then, on either supposition, the sum of the combined rotations is zero; or, to use a more convenient and suggestive form of expression, if the poles of rotation form a closed spherical polygon whose angles are respectively equal to the semirotations about the poles, the resultant rotation is zero.

This proposition is immediately derivable from the fundamental one relative to three poles, given above, by dividing the polygon into triangles by arcs, joining any one of the poles

* This proposition will be seen to be immediately demonstrable, by the comparison of equal triangles, when viewed as the converse of this other. “The arcs (or right lines) joining the correspondent extremities of the bases of two similar isosceles spherical (or plane) triangles having a common vertex, are equal to each other.”

with all the rest, or (as pointed out to me by my eminent friend Prof. W. Thomson) it becomes apparent as a particular case of a more general proposition, or representing the motion about the successive axes as effected by two equal pyramids having a common vertex at the centre of motion, of which the one is fixed in space, and the other is fixed in the revolving body and rolls over the first, so that the corresponding equal faces are successively brought into coincident apposition.

26 Lincoln's-Inn-Fields,
November 5, 1850.

P.S. To find the pole of rotation whereby PQ may be brought into the position P'Q', we may use the following simple construction.

Measure off from O the node of the great circles (or right lines) containing PQ and P'Q', two distances in the proper direction upon each (four distinct assumptions may be made), say OR and OS equal to one another and to the difference between OP and OP', then the pole of rotation required, say E, is the centre of the circle described about ROS, and the amount of rotation is the angle subtended by OR or OS at E. The writer of this paper suggests that *axis of displacement* would be a convenient term for designating the line whereby any finite change in the position of a body moveable about a fixed centre may be brought about; a geometrical theory of rotation leading to the investigation of a very curious species of correlation, now opens upon the view, the general object of which may be stated as follows:—

“Given upon a sphere or plane any curve considered as the locus of successive poles of instantaneous rotation, and the ratio of the rotation about each pole to its distance from the one that follows*, to construct the curve of the poles of displacement, and to determine the amount of rotation corresponding to each such pole.”

The discussion of this question offers a fine field for the exercise of geometrical taste and skill.

LIX. *On the Identity of Breislakite and Augite.* By EDWARD J. CHAPMAN, *Professor of Mineralogy in University College, London*†.

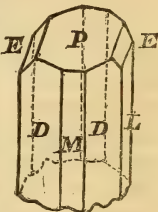
THE Breislakite is well known to occur in minute capillary crystals in certain lavas of Vesuvius, and at Capo di Bove near Rome. Its crystalline form has not hitherto

* Which by analogy may be termed the “density of rotation.”

† Communicated by the Author.

been clearly ascertained, owing to the minuteness of the crystals, and to the circumstance that usually both extremities of the prisms are engaged in the sides of the little cavities in which the substance occurs. A late examination of some specimens from Capo di Bove, in a lava apparently free from the ordinary augite crystals, enabled me, however, to detect a crystal of the accompanying form; the planes of which, though extremely minute, were very brilliant, and sufficiently defined to admit of measurement. This crystal was of the same golden brown colour as the surrounding fibres, and in every way similar to them in its general aspect, as well as in its comportment before the blowpipe, as afterwards tried. It belonged to the monoclinic system, and was a combination of the mon-axial forms P, M and L; of the diaxial prismatic form D and the diaxial pyramidal form E, or mE ; the planes of the latter being mere lines, visible from their brilliancy, but too small to reflect a distinct image.

P on M gave $106^{\circ} 18'$; P on L, 90° ; P on D, $100^{\circ} 34'$; D on D, $87^{\circ} 10'$. These values sufficiently prove that the substance was an augite*.



The measured crystal fused very readily before the blowpipe into a shining bead of the same colour as the unaltered mineral, and but very slightly attractable by the magnet: several capillary crystals behaved exactly in the same manner. With the usual blowpipe reagents I could not obtain the least indications of copper; nor have I been able to do so with any specimen of breislakite that I have yet examined. The assertion, therefore, copied from one mineralogical work into another, that this substance contains oxide of copper, is certainly incorrect. In borax the breislakite dissolves rather slowly, and still more so in microcosmic salt, the little fibres retaining their form and colour, in the latter reagent, for a considerable time—a behaviour resembling that of most varieties of augite and hornblende. The tints imparted to the glasses are solely derived from iron. Silica is also separated in the microcosmic salt bead; and with carbonate of soda, a faint reaction of manganese is obtained.

The fibres are not apparently acted upon by boiling hydrochloric acid. The supernatant liquid, diluted, and tested with sulphuretted hydrogen, does not furnish a precipitate.

* In conformity with the French crystallographers, I have considered the planes P as basal, in the usual acceptation of the term as applied to monoclinic forms. This view is supported not only by its simplicity, but by the general symmetry of at least nine-tenths of the augite crystals.

To analyse this mineral, or even to determine its specific gravity, with any approach to accuracy would be next to impossible, owing to the difficulty of separating a sufficient quantity from the lava, nepheline, &c. with which it is associated.

LX. *Note on the employment of Right Rhomboidal Prisms in Crystallography.* By E. J. CHAPMAN, Professor of Mineralogy in University College, London*.

IN the conventional position of the axes of monoclinic or oblique prismatic crystals, the three monaxial forms, *id est*, such as cut but one axis, P, M and L, necessarily constitute a rectangular prism with oblique base; P and L, and M and L, being at right angles to each other, whilst the inclination of P on M is variable. In oblique rhombic prisms the basal plane P remains the same, M and L giving place to the di-axial form D; and when P, M, L and D are in combination, as in certain augite crystals, for example, an eight-sided prism is produced, with an oblique base P. The accompanying section (No. 1) through the secondary axes, exhibits the relative positions of these forms†. Fig. 2 is a vertical section through the principal axis and the orthodiagonal; fig. 3, a vertical section through the clinodiagonal. In the latter, therefore, the point of the orthodiagonal, or the position of a side plane L, fronts the observer.

Fig. 1.

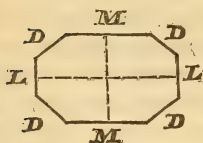


Fig. 2.

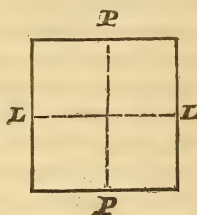
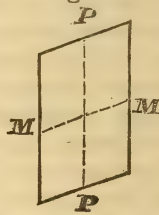


Fig. 3.



This premised, it is evident that if we place an oblique rectangular prism so that one of the planes L shall constitute the base, a right rhomboidal prism will be produced—a form frequently adopted by certain mineralogists to the exclusion of the former solid. Here, however, it is obvious that the positions of the axes can no longer be the same as in the oblique rhombic prisms; so that, in adopting this form, we must also adopt two positions for the axes of monoclinic cry-

Communicated by the Author.

† In the notation of Naumann, $P=OP$; $M, \infty P\infty$; $L (\infty P\infty)$; $D, \infty P$.

stals. This, although an unnecessary complication, would not be of very material consequence were the form to occur only in a simple or isolated state; but as it occurs far more generally in combination with oblique rhombic prisms or pyramids, the anomaly arises, that either the rhomboidal or the rhombic form becomes unavoidably placed in an incorrect position. The harmony, so to say, existing amongst the individual forms of the same crystallographic group is thus destroyed, without the substitution of any real advantage. The employment, therefore, of right rhomboidal prisms is undoubtedly objectionable.

LXI. *On the Theory of a new species of Locomotive Vessel that will diminish the ordinary resistance of the Water to one-fortieth part of its retarding power in Vessels of the same burthen.* By GEORGE WALKER, Esq., of Port Louis, France. Communicated by Sir GEORGE CAYLEY, Bart.

[With a Plate.]

GENERAL prejudice and even engineering incredulity, not many years ago, denied the possibility of vessels being propelled, unless we had the two elements of air and water to act against each other; and also affirmed, that turning the wheels of railway vehicles by steam power could never give them any useful impulse.

Now the sea is kept in continued foam by the swift prows of its tens of thousands of steam-boats; and the land, of almost every nation, is begirt with its iron roads in every possible direction; and monster-trains are every day conveying their thousands at forty and fifty miles an hour. The incredulity is forgotten; and the parties so forgetting it are quite as ready to avail themselves of these wonderful and most useful practical advantages, as if they had never disbelieved in their possibility, or opposed their nascent progress. Such, however, is the history of human invention: at every new step, first discovered by science, and then attempted to be realized by art, the same incredulity and the same opposition has to be met; but after so many practical lessons on the determined march of scientific engineering in the present age, it behoves us to examine, with competent eyes, and to be more cautious, before we reject projects that startle our present ideas of what is practicable, especially when they involve, *if possible*, an enormous field of *utility*.

The present announcement is of this class; and its theoretical basis is all that is intended to be set forth in this communication.

If the theory be sound, it will not be so in vain ; and however difficult, and perhaps remote its application may be, yet human ingenuity at length overcomes mere practical difficulties, and realizes the use of every great general principle in Nature, at their appointed time, to meet the extended wants of advancing civilization.

The ocean, bearing its heavy burdens from any one shore to every other by wind and tide, and lately by steam, has hitherto been the great means both of civilization and wealth ; but the resistance of water to vessels increasing in the enormous ratio of the squares of their velocities, has placed a limit to steam-boat speed, which can only be exceeded by an unremunerative expense of steam power : and even this is not by any means the limit of the barrier that has to be overcome ; for every increase of speed in the vessel requires an equal increase of swiftness in the paddles to overtake it, which causes the steam power expended to be in the ratio of the cubes of the velocities of the vessel. Thus double the speed requires eight times the power, and three times the speed requires twenty-seven times the power. There is no principle in nature yet known to modify this rapidly accumulating law of resistance, but that of increasing the magnitude of our vessels to as great an extent as their materials will safely permit ; for the resistances will vary as the surfaces of the prows of the vessels, and these are as the squares of any homologous dimensions ; whereas the engine power any vessel can carry is as its solid contents, and these are as the cubes of such homologous parts : hence if any vessel be double the size of another, its resistance will be only fourfold greater at any given velocity, but its floatage to carry engine power will be eightfold.

We have here all the elements of steam-boat navigation as it now stands ; and it seems probable that from eighteen to twenty miles per hour, at great cost, may be a tolerably approximate limit of speed, under every advantage our knowledge of engine power and ship-building materials can command ; and a most astonishing triumph of mechanical skill over natural impediments it is to have attained to this speed.

The law of resistance in water is the great obstacle that causes this enormous consumption of power ; and if it can be shown that there exists in nature a mechanical principle capable of reducing this expense to one-fortieth part of its present scale, its physical and moral value need scarcely be pointed out ; it would extend our railroad speed over the ocean without the cost of its iron way, and this in every possible direction within the command of a sea-girt world. Who can scan the future results of such an increased power of com-

munication between the distant races of men? Let us not, however, consider these advantages as yet within our grasp: it will be quite sufficient if the demonstration here offered of this new principle have no lurking fallacy within it: to all appearance the result seems as undeniable as that of any other mathematical demonstration when applied to mechanical power.

It has been ascertained, by the careful and satisfactory experiments of the French Academy, that the resistance to oblique surfaces moving through water by no means varies in the duplicate ratio of the sines of the angles of incidence, as has been theoretically supposed.

Fifteen boxes were made, two feet wide, two feet deep, and four feet long: one of them was a parallelopiped of these dimensions; the others had prows of a wedge form, the angle varying by 12° from 12° to 180° , so that the angle of incidence increased by 6° from one to another. When the prow was at an angle of 12° , and the angle of incidence 6° , as shown at Plate IV. fig. 3, the resistance was as 3.999 to 10.000 on the base or front surface of the parallelopiped, say practically as 4 to 10.

By the same authority, the resistance to one square foot, French measure, moving with the velocity of 2.56 feet per second, was very nearly 7.625 pounds French. The resistances increased very correctly as the squares of the velocities; and reducing these to English measures, a square foot moving perpendicular to itself in river water, receives one pound resistance when moving with a velocity of 1.01, say 1 foot, per second.

Let AB, fig. 1, be a plane moving perpendicular to itself in water from A to H, and let the velocity be represented by the line AH: the resistance to AB will be as $AB \times AH^2$. If the plane AB move towards E, parallel to itself, with a velocity $Ah = AH$, the resistance to its perpendicular section AH will be expressed by $\frac{AH}{AB}$. But experiment has proved that the resistance to AB is $\frac{2}{5}$ of that to its perpendicular AH, viz. $\frac{2AH}{5AB}$.

If AB move to E with the velocity AE, the resistance will be $\frac{2AH}{5AB} \times AE^2$; and the power required to overcome this resistance of AB towards H, as $\frac{2AH}{5AB} \times AE^2 \times AE$ is to $AB \times AH^2 \times AH$.

Let the plane AB be ten feet long by one broad, and let the distance from A to H be one foot; then if it be depressed,

so as to arrive at H in one second, according to the experiments quoted, the resistance will be ten pounds.

If it move in the direction AE, as before, and AH be one-tenth part of radius, the resistance to the section AH will be one-tenth of that of AB in its motion towards H; and if AB move towards E, the resistance to it will be $\left(\frac{4}{10}\right)\frac{2}{5}$ of what it was against AH, viz. $1 \times \frac{2}{5} = \frac{2}{5}$.

If AB be moved with the velocity AE=10AH, the resistance will be

$$10 \times 10 \times \frac{2}{5} = \frac{200}{5} = 40;$$

and therefore the power to move AB with the velocity AE will be $40 \times 10 = 400$; that is, forty times the power required to move AB to H. Q. E. D.

It is not intended in the present communication to enter upon the various methods that have occurred for carrying out this principle of evading the direct resistance of water, by making the whole displacement of it perpendicular, or nearly so, to the line of the vessel's path; but to render the idea more palpable and distinct, conceive a succession of hollow floats connected together by hinges into an endless chain passing round drums at each end of a frame (see fig. 2), and the lower line floating on the water, and kept, by the nature of the joints, from bending upwards between the drums, aided, if necessary, by intermediate ones. If the drums are made to revolve by steam power or otherwise, new floats are laid down in front, and others are taken up behind in continued succession; and the act is like that of a continue railway over boats. Each float is placed in the water and taken out again without changing its position horizontally; and this, not immediately under each drum, but with the moderate velocity due to an inclined plane; being kept above the water at each end like the prow and stern of a gondola. To gain sufficient buoyancy, a much longer line of floats, depressed by an intermediate succession of drums, may be required; but such considerations form no part of the present essay. It is necessary, however, to observe that the floats have hitherto been only considered as respects the resistance of their lower surface against the water as a plane of certain dimensions moving with a given velocity: these floats will, however, require considerable force to depress them and displace their bulk of water; but being depressed and withdrawn by a slow action, very nearly all the force absorbed by the immersion will be restored by their own buoyant action in emerging; so that this small loss of power

need scarcely be taken into consideration as any set-off against a principle promising a gain of forty to one.

The following additional demonstration of this principle having been submitted to Mr. Walker, he requested that it may be added as corroborative of his views.

If the French experiment of the wedge form, represented by fig. 3, were made only one foot deep, by two feet wide at the base, that half of the prow below the centre line would correctly represent the plane AB, fig. 4; and if both were moved horizontally with the velocity of ten feet per second, as from AB to CD, the plane AB would just meet half the resistance of the prow.

Experiment has shown that the two square feet of base to the prow would, if the prow were absent, at that velocity receive 100 pounds resistance each, = 200 pounds; and that, with the prow, the resistance would be $\frac{4}{10}$ ths of this, or 80 pounds. Thus one-half, or 40 pounds, would be the horizontal resistance of the plane AB, moving ten feet per second, which is equivalent to 400 pounds moved one foot per second.

As the resistance varies as the squares of the velocities, if the plane AB were placed horizontally, as represented by AE, and then depressed in one second to DB (being one foot), each of the ten square feet would only receive one pound of resistance, and the whole would be ten pounds moved one foot per second, which is just one-fortieth part of the power required in the former case. Q. E. D. G. C.

November 4, 1850.

LXII. *On the Untenableness of the received Theory of Newton's Rings.* By E. WILDE*.

A DESIRE to investigate the colours of thin plates more closely induced me to have an instrument prepared, by which I am enabled actually to measure the diameters of the coloured rings of Newton to the ten-thousandth part of an English inch, and to attain by estimation an accuracy of one hundred-thousandth of an inch; the same instrument determines the approximation of the glasses to the millionth of an inch. It is therefore capable of a greater degree of exactness than even the spherometer of Biot. I will name it a gyreidometer.

Some years ago an instrument for the exhibition of Newton's rings was invented by Jericau in Sweden; it was named a gyreidoscope by its inventor. The instrument however

* Translated from Poggendorff's *Annalen*, vol. lxxx. p. 407, July 1850.

could not be used for the purposes of measurement, and hence I found myself compelled to resort to other principles in the construction of my own.

In the description which he has given of his gyreidoscope, Jericau dissents from the theory of Newton's rings, which up to the present time has been accepted; inasmuch as experiment has taught him, that, by pressing the glasses very strongly together, the centre, instead of appearing dark, as asserted by Newton, actually becomes bright. He endeavoured to explain the matter by referring it to the repeated diffusion of colours within a circle. I must however confess that I am unable to attach any clear meaning to his explanation.

Prompted, however, by this remark of Jericau's, I was induced to undertake a closer investigation of the subject as soon as I found the gyreidometer in my possession. I have not been able to corroborate the statement, that by pressing the glasses very strongly together the centre becomes bright. I have found that the first result of the pressure is a dark central spot, which is formed at the summit of the convex lens; but by increasing the compression and bringing the glasses more closely into contact, the black spot expanded into a circle with a bright centre; in the middle of this bright patch a dark spot again appeared as the pressure was increased, and it thus continued changing according as the length of the paths of the respective rays reflected at the upper and lower limit of the layer of air differed from each other by an odd or even number of semi-undulations, until finally a constant dark central spot was obtained, which it was impossible to supersede even when the glasses were so forcibly pressed, that the upper one, which was a quarter of an inch in thickness, actually bent under the force. As the spot increased in size, the surrounding curves diverged more and more from the circular form. By ordinary daylight, as might be expected, the change from dark to bright, which accompanied the approach of the glasses, could not be perceived; a variety of colours exhibited themselves one after the other, until, finally, upon very strong pressure, the dark central spot was obtained. A small number only of rings were visible when viewed with common light; but with homogeneous light the field of view of the microscope appeared covered with many hundred curves, alternately dark and bright.

But although I find myself thus unable to coincide with the assertion of Jericau, a careful examination of the rings of Newton with the gyreidometer has furnished me with other grounds of dissent from the generally received theory which

has been deduced from the theory of undulation by Young, Fresnel, Poisson, Herschel, Airy, and others. My grounds of dissent are as follow :—

1. It is theoretically impossible that a reversion of the æther-vibrations, reflected at the lower limit of the one glass as compared with those reflected at the upper limit of the other, can take place when there is no layer of air between the glasses.

2. If, in order to preserve the received theory, a layer of air so thin that its depth is very small even in comparison with the length of an undulation, be assumed to exist at the point where the dark spot commences to show itself, this could not account for the continuance of the spot after the glasses have been pressed so strongly together that no air can possibly exist between them. This assumption therefore does not help us; the central spot cannot be referred to the interference of the rays reflected at the respective limits of the glasses, though this is the explanation which has heretofore been accepted.

3. In reflected daylight the central spot appears a deep black. Its origin, therefore, even should we grant the existence of an intermediate air-layer, can by no means be referred to the principle of interference; for it is well known, that, owing to the difference in the lengths of the respective undulations, the interference of rays compounded of all colours must result in a tinting more or less vivid. Since therefore the central spot cannot be referred to the existence of a thin layer of air, nor for the reason first assigned be referred to the principle of interference, we are driven to seek a different explanation of the phænomenon from that heretofore given.

4. Because, with homogeneous light, if the distance of the glasses from each other be equal to nought, and a reversion of the vibrations consequently impossible, the phases of the rays reflected from the two glasses at the point of contact must correspond, and a dark spot cannot be the consequence.

5. Because, in the case under consideration, the phænomena of colour which are exhibited in all analogous cases, among which may be reckoned the figures due to diffraction, are entirely absent.

From all these grounds it follows, that the middle of the ring-system, when the distance between the glasses is nought, cannot appear dark in reflected light; but that, in direct opposition to all assertions heretofore made, it ought to appear bright.

The shortest proof for the so-called law of Young, I find in Fresnel's expression for the velocity of oscillation of a reflected ray polarized in the plane of incidence, and of a reflected ray

polarized perpendicular to the plane of incidence ; the former (that is, of the ray in which the oscillations are at right angles to the plane of incidence and to the direction of the ray) has the following value :—

$$-\frac{\sin (i-r)}{\sin (i+r)}, \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

and the latter (that is, of the ray in which the oscillations are parallel to the plane of incidence and perpendicular to the direction of the ray) the value*—

$$\frac{\tan (i-r)}{\tan (i+r)}, \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

At the upper limit of the layer of air, the angle of incidence i is smaller than the angle of refraction r , the passage in this case being from glass to air ; the expression (1.) is therefore positive. At the lower limit of the air-layer, on the contrary, the angle i is greater than r , the passage in this case being from air to glass ; and the same expression is consequently negative. In the same way the expression (2.) changes its sign according as it refers to the one or the other limit of the air-layer ; at the upper limit it is negative, and at the lower limit it is positive. A change of sign, however, in the velocity of oscillation is always accompanied by a reversion of the direction of oscillation. Hence, when a natural (unpolarized) ray, which as regards intensity may be considered as composed of the two polarized rays mentioned above, is reflected at the lower boundary of the layer of air, a reversion of the æther oscillations, as compared with their direction after reflexion from the upper boundary, must take place, which reversion, as regards the intensity of the reflected light, has the same effect as if the difference of the paths traversed by both rays was half an undulation (or in general an odd number of semi-undulations) greater or less than it really is ; *this however under the sole condition that a layer of air exists between the two glasses*. If therefore a difference of half an undulation has been heretofore assumed at the point where no air exists between the glasses, the said assumption has been made in direct opposition to the theory of undulation.

If, on account of the reversion of the oscillation, I assume the difference of the paths traversed by the interfering homogeneous rays half an undulation λ (or an odd number of half undulations) greater or less than it really is, then the *simplest* expression for the intensity of the reflected light I find to be

$$J = 4a \sin^2 2\pi \frac{d \cos r}{\lambda}, \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

* Poggendorff's *Annalen*, vol. xxii. p. 90.

where a represents the quantity of light reflected at the upper or lower boundary of the layer, d the depth of the layer, and r the angle of refraction from glass to air. For the transmitted light, on the contrary, where no reversion of the vibration takes place, the intensity is

$$J' = 1 - 4a \sin^2 2\pi \frac{d \cos r}{\lambda}, \quad . \quad . \quad . \quad (4.)$$

so that the intensities are complementary, the sum of both being the intensity 1 of the incidental light.

For reflected light, we obtain from (3.) the maxima of intensity for

$$d = \frac{\lambda}{4 \cos r}, = \frac{3\lambda}{4 \cos r}, = \frac{5\lambda}{4 \cos r}, = \frac{7\lambda}{4 \cos r}, \quad . \quad . \quad .$$

and the minima of intensity for

$$d = 0, = \frac{2\lambda}{4 \cos r}, = \frac{4\lambda}{4 \cos r}, = \frac{6\lambda}{4 \cos r}, \quad . \quad . \quad .$$

because for the first series $J = 4a$, and for the second $J = 0$. For the transmitted light, we obtain from (4.)

the maxima for

$$d = 0, = \frac{2\lambda}{4 \cos r}, = \frac{4\lambda}{4 \cos r}, = \frac{6\lambda}{4 \cos r}, \quad . \quad . \quad .$$

and the minima for

$$d = \frac{\lambda}{4 \cos r}, = \frac{3\lambda}{4 \cos r}, = \frac{5\lambda}{4 \cos r}, = \frac{7\lambda}{4 \cos r}, \quad . \quad . \quad .$$

because for the first series $J' = 1$, and for the second series $J' = 1 - 4a$. Now as the diameters or radii of the coloured rings are proportional to the square roots of the depths of the layers of air, the diameters or radii of the maxima in the case of reflected light bear to each other the ratio $\sqrt{1} : \sqrt{3} : \sqrt{5} \dots$; and those of the minima the ratio $\sqrt{0} : \sqrt{2} : \sqrt{4} \dots$. With transmitted light, on the contrary, the ratios of the maxima will be $\sqrt{0} : \sqrt{2} : \sqrt{4} \dots$, and those of the minima $\sqrt{1} : \sqrt{3} : \sqrt{5} \dots$.

This is the manner in which Newton first expressed the law, and in the same manner it has heretofore been deduced from the undulation theory. It is however so far from agreeing with the same, that the law, to be rendered applicable to the centre of the ring-system, would require entire reversion.

The rings nearest the centre begin to lose their circular shape, and to assume one more or less elliptical, immediately before the appearance of the constant dark spot, *and while the centre is still bright*. There can be no doubt that actual

contact has taken place, and that the distance between the glasses is already $=0$ when the shape of the rings has begun thus to alter, inasmuch as this alteration manifestly intimates an actual pressure of the glasses. Then, however, the first bright ring must appear as the uninterrupted continuation of the bright central patch, because the first dark ring is not formed until for $\cos r = \cos 0^\circ = 1$ the depth d of the layer of air is $\frac{2\lambda}{4}$, the difference of the paths of the interfering rays (taking into account the journey forward and back through the air-layer and the reversion of the vibration) being at this place $= 2\frac{2\lambda}{4} + \frac{\lambda}{2} = \frac{3\lambda}{2}$, and the light consequently destroyed. When this is the case, we have with reflected light, the maxima for

$$d=0 \text{ and } \frac{\lambda}{4 \cos r}, = \frac{3\lambda}{4 \cos r}, = \frac{5\lambda}{4 \cos r}, = \frac{7\lambda}{4 \cos r}, \dots$$

and the minima for

$$d = \frac{2\lambda}{4 \cos r}, = \frac{4\lambda}{4 \cos r}, = \frac{6\lambda}{4 \cos r}, = \frac{8\lambda}{4 \cos r}, \dots$$

With transmitted light, on the contrary, the maxima for

$$d = \frac{2\lambda}{4 \cos r}, = \frac{4\lambda}{4 \cos r}, = \frac{6\lambda}{4 \cos r}, = \frac{8\lambda}{4 \cos r}, \dots$$

and the minima for

$$d=0 \text{ and } \frac{\lambda}{4 \cos r}, = \frac{3\lambda}{4 \cos r}, = \frac{5\lambda}{4 \cos r}, = \frac{7\lambda}{4 \cos r}, \dots$$

from which it follows, that with reflected light the radii of the successive bright rings are in the ratio of $\sqrt{1} : \sqrt{3} : \sqrt{5} \dots$, and those of the dark rings in the ratio of $\sqrt{2} : \sqrt{4} : \sqrt{6} \dots$. With transmitted light, however, the radii of the bright rings are in the ratio $\sqrt{2} : \sqrt{4} : \sqrt{6} \dots$, and of the dark rings in the ratio $\sqrt{1} : \sqrt{3} : \sqrt{5} \dots$; the middle of the system, up to the points where the layer of air reaches a depth of $d = \frac{2\lambda}{4}$, appearing at the same time in maximum with reflected light, and in minimum with transmitted light.

The measurements I have made with the gyreidometer, in which were placed a plane glass and a convex glass of 360 inches Eng. radius, are my security against error here. When using reflected light, the glasses were pressed together until the circular form of the rings began to alter, the middle of

the system however being still bright, with the homogeneous light of a lamp of alcohol and chloride of sodium* for the angle of incidence $39^{\circ} 41'$ (the angle of refraction r from glass to air), I found, by taking in each case the mean of repeated measurements, the radii to be—

Reflected Light.

Radii of the

first,	second,	third,	fourth bright ring.
...	0.1244" Engl.	0.1602"	0.1900"
first,	second,	third,	fourth dark ring.
0.1021" Engl.	0.1439"	0.1765"	0.2040"

These values however need a sensible correction, inasmuch as the radii of the rings, on account of the refraction of the upper plate, a quarter of an inch thick, appear smaller than they really are at the upper limit of the air-layer. The mean exponent of refraction necessary for the ascertaining of these corrections, I obtained according to the method of Precht†, from a series of ten measurements. The microscopic lens being 2.91 English inches distant, the following corrections are obtained:—

Reflected Light.

Corrections for the

first,	second,	third,	fourth bright ring.
...	0.0070"	0.0091"	0.0106"
first,	second,	third,	fourth dark ring.
0.0056"	0.0081"	0.0099"	0.0113"

These corrections being applied to the radii found above, and the radius of the first bright ring being calculated from the second by means of the proportion $1 : \sqrt{3}$, we obtain, finally,—

Reflected Light.

Radii of the

first,	second,	third,	fourth bright ring.
0.0758"	0.1314"	0.1693"	0.2006"
first,	second,	third,	fourth dark ring.
0.1077"	0.1520"	0.1864"	0.2153"

* This light is not completely homogeneous, but a mixture of yellow and violet, as I found by decomposition with a prism; so that it deserves to be called orange rather than yellow. It is however sufficiently homogeneous for the purpose to which it has been here applied.

† Practical Dioptrics. Vienna, 1828, p. 127.

which values are so exact, that, when set down in the form of a proportion with the square roots found above, the product of the extremes agrees with the product of the means to at least four places of decimals. Thus, for example, for the first and third dark rings, the following proportion ought to hold good, $0.1077^2 : 0.1864^2 = 0.01159 : 0.03474 = 2 : 6 = 1 : 3$, which it actually does, the product of the extremes agreeing with the product of the means to four places, viz. 0.0347.

The laws deduced from the undulation theory for the reflected light are thus confirmed by experiment, without its being necessary to press the glasses so strongly together as to cause the dark central spot to exhibit itself; and this is the point which it was my object to prove.

Another consideration which supports the truth of my assertion is, that by all analogous phænomena of colour where, when homogeneous light is used, an alternation of dark and bright is observed, *the middle being bright*, the first minimum occurs where the paths traversed by the interfering rays differ by at least an entire undulation. Thus, for example, in the alternation which occurs when light, after having passed through a narrow opening, is received upon an opaque screen, the middle, where the difference of path is nothing, is always bright, its intensity being $= 1$. The space however remains also bright when the difference amounts to half an undulation, its intensity at this point being 0.4053, and it is not until the difference amounts to an entire undulation that the first minimum occurs*. The resemblance is closer still when the diffraction is caused by the rays passing through a small circular opening, in which case the middle is also bright, and the first dark ring occurs where the difference of the paths traversed by the rays is 1.220λ †. It is, on this account, impossible to determine the difference of path corresponding to the first maximum ring, which appears as a continuation of the bright centre; this difference must be obtained from the expression of intensity (3); and for the same reason I have been unable to ascertain by measurement the radius of the first bright ring, being obliged also to resort to the expression (3.) to obtain it. Notwithstanding, however, that in all these related phænomena, where the centre is bright, the first minimum does not occur where the difference is half an undulation, still up to the present time it has been assumed, in the case of *transmitted light*, that the centre of Newton's ring-system is bright, and that the first dark ring nevertheless occurs where the depth of the layer of air is one-fourth of an undulation, that is to say, where the difference of paths amounts to half an un-

* Poggendorff's *Annalen*, vol. lxxix. p. 206.

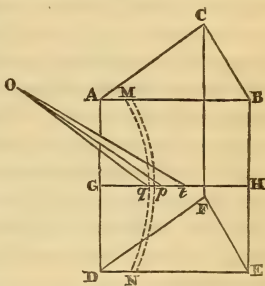
† Ibid. p. 224.

dulation, for in this case the vibrations are not reversed. No analogy therefore exists between the theory of Newton's rings heretofore received, and that which explains other phænomena of a similar nature.

The error is evidently due to the circumstance, that, trusting to the accuracy of Newton's observations, the formula of intensity (3.), which applies solely to the case where a reversion of the æther vibrations may be assumed, has been extended to the case where the distance being 0, no reversion is possible. It may be remarked, further, that the depth 0 has never been introduced into the proportion, nor could it be so introduced, as a fourth proportional infinitely great would be the consequence. It is thus easy to conceive how, neglecting to test the original observations of Newton, the same error might be repeated through centuries.

With regard, finally, to the origin of the central spot, which by daylight appears black, but by homogeneous light, on account of the dull background, does not appear darker than the rings caused by interference, my opinion coincides with that of Jericau, *that the spot is caused by transmitted light in the same manner as the spot upon a mirror when a little of the metallic coating is removed from behind.* The part from which the coating has been removed appears dark in comparison with the surrounding bright reflecting surface. I however agree with this notion, not as a probable conjecture, but as an undeniable fact. The grounds of this assertion are the following :—

Let DEC represent a glass prism, O the place of the eye, and GH a line parallel to the ends. A refraction of red light is first possible at t when the angle OtG is $=49^{\circ} 2'$, or the angle of incidence at $t=48^{\circ} 58'$; and a refraction of the violet in q first occurs when the angle OqG $=50^{\circ} 7'$, or the angle of incidence $=39^{\circ} 53'$. Let p be the place at which the middle rays are refracted.



Between t and BE no ray will be broken at the base DB, and hence in this portion a total reflexion will take place. Between t and p , red, orange and yellow rays can pass through; and, on account of their absence, the light reflected from this portion will be diminished in intensity. Between p and q all the other rays, blue and violet excepted, are allowed to pass; here therefore the intensity of the reflected light will be still less, while in the vicinity of q , where the blue and violet have

not yet found a passage, the bluish-bow MN is observed, which Newton was the first to notice and explain. In the portion of the base between MN and AD, where all the rays are refracted, the reflected light is for this reason very dull.

Having laid the basis DB of the prism upon the summit of a very flat convex lens, I pressed both glasses forcibly together, and inclined the eye towards the base, so that I could see the place where the glasses touched between q and AD. Here were exhibited a considerable number of rings, such as those observed in daylight with two very flat convex lenses; a complete central spot of deep black was visible at the same time. The eye being bent still lower while the glasses remained unaltered, I observed the place of contact between q and p ; the central spot appeared no longer black, but a most vivid green. When the eye was inclined still more, colours of the brightest yellow, orange, and at last red, were exhibited successively, until finally, when the eye was sunk still lower, so that the place of contact came into the vicinity of total reflexion, the deep black spot again appeared complete as before.

This experiment is decisive. It proves *that the hue of the central spot depends upon transmitted light, and for ever sets aside the possibility of the assumption, that the origin of the spot is to be sought in interference.* The colours thus obtained are perfectly homogeneous; and hence, if the idea of interference could in the least be admitted, the central spot must remain invariably black. This however is not the case. The colours green, yellow, orange, which the central spot assumes, are as vivid and pure as those obtained from the best flint-glass prisms, which is especially evident when the glasses are laid upon black paper and the light prevented from falling upon the lens itself. Of this I convinced myself by placing an opake screen on that portion of the lens turned towards the light which was uncovered by the prism, and thus holding back those rays which could fall upon the base from below. Had the lens a stronger convexity, the colours of the central spot, as may be readily conceived, would not be sufficiently separated.

Although this experiment renders further proof of the correctness of my assertion superfluous, I will nevertheless introduce a few others which have furnished me with the same results.

Upon the side of the prism turned towards the light, I laid a piece of black paper, so that light could only be received through the lens from below. The lens and prism being pressed more forcibly together, the place of contact, when observed

through the side of the prism turned towards the room, appeared like an uninterrupted and very bright opening passing through both glasses, while the surrounding portion of the base of the prism was dull and dim. This experiment proves the uninterrupted passage of the rays through the centre where the light falls from above upon the uncovered side of the prism.

When two lenses placed together were laid upon white paper, the hue of the central spot appeared less intense than when black or merely dark-coloured paper was used, and the spot was not at all visible except when the light was obliquely reflected. At smaller angles of incidence it vanished totally, and appeared as a white circle when I looked vertically down upon the place of contact of the glasses, the ring-system being visible at the same time. If, however, the darkness of the central spot was caused, as heretofore supposed, by interference, by a self-destruction of the light, the spot must continue dark, even when the glasses are laid upon a white ground, under all possible angles of incidence, and the light proceeding upwards from the paper could not cause the place of contact to appear white, as it actually does.

Having united the summits of two convex lenses with Canadian balsam, I laid them on a black or dark-coloured surface, and observed, by all incidences of the rays, that the place on which the balsam lay exhibited the same deep black as when two glasses are pressed together in daylight. When, however, the lenses were laid upon white paper, the black, as in the case of the pressed glasses, was less intense, and the spot also disappeared when small angles of incidence were used. Although the continuity of the glasses in this case was only partially effected by the balsam, still every experiment made with the lenses thus united pronounced decisively that the darkness of the central spot is to be referred exclusively to the free transmission of the light at the place of contact.

In the first experiment, it is observed that in the neighbourhood of total reflexion, on the base of the prism, the central spot is not surrounded by rings. Rings cannot show themselves, because at the place where they ought to appear no rays are transmitted which after reflexion from the lens could interfere with those reflected from the base. The central spot, however, appears black, because as no air exists between the glasses at the point of contact, no total reflexion is here possible, the light being permitted to pass on. For were we to assume the existence of a layer of air at the place of contact, the light must undergo total reflexion at this point also, and hence the central spot must appear as bright as the surround-

ing portion. Since, therefore, the presence of a layer of air cannot be here assumed, the darkness of the central spot—even setting aside the consideration that black can never result from the interference of heterogeneous rays—cannot arise from interference, because the latter without an air-layer is impossible.

The fuller development of the views expressed in the foregoing pages I reserve to a future memoir. In submitting my divergence from received opinions to the judgement of those who understand the subject, my sole object is to bring all optical action, no matter of what particular kind, into coincidence with the theory of undulation; so that the harmony of our thoughts with the divine order of Nature, at least in this department of science, shall remain undisturbed.

LXIII. *Note to a former paper "On an alleged proof of the 'Method of Least Squares.'"* By R. L. ELLIS, late Fellow of Trinity College, Cambridge.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ALLOW me to correct an error in my letter to Professor Forbes, published in your last Number. The Edinburgh reviewer, on whose proof of the method of least squares I was commenting, says that the most probable position of the wafer is the centre of gravity of the shot-marks; of course on the supposition that in this, as in all other cases, the probability of a deviation or error r is equal or proportional to a certain constant base raised to the power $-r^2$.

Now, admitting this supposition to be true, the centre of gravity is not the most probable position of the wafer. But, on the contrary, if the function mentioned at the close of my former communication, viz. $2h^2e^{-h^2r^2}rdr$, expresses the probability of an error r , then the centre of gravity is the most probable position. I thus not only omitted to notice that the reviewer's conclusion would not follow from his own hypothesis, but by this omission was led to introduce an error of my own.

It is unnecessary to trouble you with a proof of what I have now said, as the matter does not affect the general question.

I am, Gentlemen,

Your obedient Servant,

R. L. ELLIS.

Brighton, Nov. 7.

LXIV. *On a Deduction of Ohm's Laws, in connexion with the Theory of Electro-statics.* By G. KIRCHHOFF*.

IN the deduction of his laws of galvanic currents, Ohm set out with certain assumptions regarding electricity which are not in conformity with those it has been necessary to make in order to explain electro-static phænomena; he assumes that the electricity in a conductor is at rest when it is distributed throughout the latter in a state of uniform density. Now although it must always appear desirable to determine the laws to which electrical currents are subject, by considerations connected with the theory of electro-statics, this becomes absolutely requisite to enable us to produce a satisfactory theory of experiments, in which both electricity in motion and electricity at rest are concerned,—experiments similar to those made by M. Kohlrausch upon the closed circuit with the condenser and electrometer†. My present object is to show how Ohm's formulæ may be deduced from the electro-static laws of the mutual repulsion of electrical atoms, when certain assumptions referring to questions in the theory of electro-statics, which have remained perfectly open, are brought to bear.

When electricity is communicated to a conductor, it will assume a state of equilibrium, when the forces exerted by the free electricity upon an electric atom existing in any part of the interior of a conductor mutually neutralize each other. This occurs when the potential of the total amount of free electricity in relation to a point within the conductor remains constant. Theory shows us that this can only be the case when the free electricity has become arranged in a particular manner upon the surface of the conductor.

When two conductors of different kinds, as a piece of copper and a piece of zinc, which separately contained no free electricity, are brought into contact with each other, one conductor becomes positively, whilst the other becomes negatively electrical. The electricity excited at the point of contact soon assumes a state of equilibrium; in it the potential of the total amount of free electricity must necessarily remain constant with regard to all points of each of the two conductors: hence it follows that free electricity cannot exist within the conductor, and that it must be situated solely upon its surface; one portion of the electricity will remain at the surface of contact of the two conductors, whilst another covers its free surface.

* From Poggendorff's *Annalen*, No. 12, 1849.

† Poggendorff's *Annalen*, vol. lxxviii. p. 1.

The potential of all the free electricity is constant with regard to all parts of each of the conductors: its value, however, will be different in the case of the first conductor from that of the second; for theory teaches us, that if its value were the same in both conductors, there should be no free electricity present, inasmuch as the sum of all the free electricity is $=0$. Now as regards the difference between the two values of the potential in the two conductors, this might depend upon the nature of the material of which the two conductors were composed, and their form. I shall assume that it is independent of the latter, and is that magnitude which is known as the tension of the two bodies. Let u denote the potential of the entire amount of free electricity in regard to a point in the first conductor, and u_2 the same in regard to a point of the second conductor; both u and u_2 must then be constant; if then $U_{1,2}$ denote the tension of the two bodies, we must have

$$u_1 - u_2 = U_{1,2}.$$

If we imagine several conductors, say three, so placed in contact that the first conductor touches the second, and this the third conductor, the electricity in them may always assume a state of equilibrium. If we again denote the potential of the total amount of free electricity in any point of the first conductor by u_1 , for one in the second by u_2 , and for one in the third by u_3 , and further the tension between one and two by $U_{1,2}$, that between two and three by $U_{2,3}$, it is essential to the existence of a state of equilibrium that each of the three magnitudes, u_1 , u_2 , and u_3 be constant, and that the equations

$$u_1 - u_2 = U_{1,2}$$

$$u_2 - u_3 = U_{2,3}$$

be satisfied. But if we assume that the conductors 1, 2 and 3 have been so placed in contact that each of them comes into contact with the two others, electric equilibrium cannot possibly always exist in them. Should equilibrium exist, each of the magnitudes u_1 , u_2 , and u_3 must be constant, and the equations

$$u_1 - u_2 = U_{1,2}$$

$$u_2 - u_3 = U_{2,3}$$

$$u_3 - u_1 = U_{3,1}$$

must be satisfied. These equations, on addition, produce

$$0 = U_{1,2} + U_{2,3} + U_{3,1};$$

thence the tensions of the three conductors must satisfy this

condition to allow of the possibility of electric equilibrium; the condition is satisfied when the three conductors belong to the so-called tension series.

We have next to examine what will occur when this condition is not satisfied. At a particular moment the distribution of the free electricity in the system will be a certain one; I leave it undetermined whether this free electricity exists only at the surface of the conductor, or whether it has penetrated into its interior. Let its potential in regard to any point of one of the conductors be u ; this u is not constant, but a function of the coordinates of the point to which it relates; hence the forces which are exerted by the free electricity upon a particle of electricity existing at any spot within the conductor will not retain a state of equilibrium, but produce a definite resultant. Let us imagine the existence of an element of space, v , within the conductor, and let us denote the above resultant for any point in v by R . If v contains no free electricity, the neutral electric fluid contained in it becomes decomposed; the positive electricity will be moved in the direction of R , the negative in the opposite direction; the quantities of positive and negative electricity excited in the element v , and also their velocities, must therefore be the same. I shall assume that the quantity of either fluid, which is moved in a unit of time through a section of v , perpendicular to the direction of R , and the magnitude of which may be denoted by $d\omega$, is $=d\omega k R$, in which k denotes the conducting power of the substance. To determine what takes place when v contains free electricity, I shall assume that no motion of the electric fluids can occur in a conductor, except when equal quantities of the two electricities pass in opposite directions through each surface-element of it simultaneously. Hence it follows, even when v contains free electricity, that as much positive electricity passes in a unit of time through $d\omega$ in the direction of R , as negative electricity in the opposite direction. As regards the quantity of the electricities flowing through $d\omega$, I assume that it is again $=d\omega k R$.

If to these assumptions, most of which have been already put forth by Weber in his electrodynamic measurements, we further assume that the difference in the values of the potential of the total amount of free electricity in the case of two points lying in immediate proximity to each other on the proximal and distal sides of the surface of contact of two conductors remains the same, whether a current flows through the conductor, or the electricity is at rest in them; we arrive, on the supposition that the electric condition of the system has become stationary, at the same equations for the po-

tential of the free electricity as those given by Ohm's expression of the electric force, *i. e.* the density of the electricity.

In fact, if we denote the normal of the element by dw , that having the direction of R by N , then

$$R = \frac{du}{dN},$$

hence the quantity of positive or negative electricity flowing through dw in a unit of time, is

$$= -kdw \frac{du}{dN}.$$

The same expression is obtained for this quantity by Ohm's method, if u be used to denote the electroscopic force*. But we may conclude from this expression, without entering into the signification of u , that when the condition of the system has become stationary, u must satisfy the differential equation

$$\frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} + \frac{d^2u}{dz^2} = 0;$$

and for each point of the free surfaces of the conductor, the limitary condition

$$\frac{du}{dN} = 0;$$

and further, that the equation

$$k \frac{du}{dN} + k, \frac{du_1}{dN} = 0;$$

applies in the case of every point of the surfaces of contact of two bodies.

To these conditions, both as regards Ohm's proposition and those we have enumerated, must be added, that in the case of every point of the same surface of contact, $u - u_1 =$ the tension of the two bodies. Thus the same equations are obtained for the magnitude u by both propositions. As regards the currents which are determined by the differential quotients of these magnitudes, we consequently obtain the same results from whichever we start. But different results are obtained in regard to the distribution of the free electricity in the circuit. According to Ohm, the value of u at every part of the system directly gives the density of the electricity, which is not the case in the view we have developed, from which, on the contrary, it follows that even in the closed circuit free elec-

* Poggendorff's *Annalen*, vol. lxxv. p. 191. We have used the word tension here to denote Ohm's electroscopic force.

tricity can only exist at the surface of the conductor. Thus as u within one of the conductors satisfies the equation

$$\frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} + \frac{d^2u}{dz^2} = 0,$$

u must be a potential of masses which are situated externally to this conductor. But u is a potential of all the free electricity; hence no part can be situated in the interior of the former conductor, nor in any part of the interior of any conductor.

The considerations we have laid down hold good whatever the number, the form, and the arrangement of the conductors may be which are placed in contact; they also hold good in that case in which one plate of a condenser is placed in contact with a point of a closed circuit, and hence afford the theory of experiments similar to those of M. Kohlrausch. The results which they yield agree perfectly with those of this experiment.

The considerations we have laid down are based upon the electro-static law of the action of electric particles. Neither Ampère's electro-dynamic phænomena, nor the phænomena of induction, can be explained by this law. Weber has discovered a more general law, by which he has succeeded in explaining these phænomena; a law, in the expression of which the relative velocity of the particles, whose action upon each other is under consideration, is introduced, and which passes into that of electro-statics, when this velocity disappears. In bringing the various fields of the theory of electricity under a single point of view, we must therefore aim at deducing the laws of the currents in the closed circuit from Weber's law. This deduction appears to be difficult; still it is easy to prove, *à posteriori*, that the idea regarding the currents, to which the admission of the electro-static law has led, is also in conformity with Weber's law, when a certain hypothesis is called in aid, viz. that hypothesis, according to which, on calculating the force which a separation of the two electricities produces in the element of space v of one of the conductors, the electricities in v must be regarded as at rest. There is nothing opposed to this view, when we bear in mind, that the motion of the electricity in one conductor only passes from molecule to molecule; so that every particle of electricity finds a point of rest in a molecule which it reaches. Adopting this view, it may readily be granted that the quantity of electricity which is transferred from one molecule to a neighbouring one is only occasioned by the forces which are exerted upon the particles of electricity, whilst they are still in a state of rest in the former

particle, but not by the forces, which act upon it, whilst they are passing to the following molecule. As regards Weber's theory of induction, it is unimportant whether this assumption is made or not. If it be made, and the currents in the circuit be regarded generally as in accordance with the view of the electro-static law, it is a matter of indifference in regard to the magnitude and the direction of the force which tends to separate the electricities in the element v , and therefore in regard to the electro-motor force, as Weber calls it, whether we start from the electro-static or Weber's law. The difference which might possibly occur must therefore arise from the forces exerted by the electricities flowing in the other parts of the system; and these forces, according to what Weber has pointed out, do not contribute to this electro-motive force, inasmuch as the currents are constant, and convey equal quantities of both electricities with the same velocity in opposite directions.

LXV. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 233.]

Feb. 11, **A** PAPER was read by the Master of Trinity, "Criticism of Aristotle's account of Induction." 1850.

The passage criticised was *Analyt. Prior.* 11. 25, and is by Aristotle illustrated by this example. Elephant, horse, mule, &c., are long-lived; but elephant, horse, mule, &c. have no gall-bladder. If we suppose that the latter proposition may be converted and put in this form, "all animals which have no gall-bladder are as elephant, horse, mule, &c.," we may draw the conclusion that all animals which have no gall-bladder are long-lived. This convertibility and generalization of the second proposition are the necessary conditions for translating induction into syllogism. And Aristotle really contemplated such a generalizing induction. He did not contemplate what has been called *inductio per enumerationem simplicem*, which is really no induction at all. This was shown to be so by reference to the case, often used as an example of induction, of the inference of Kepler's laws from the observation of the separate planets. It may be objected that the reasoning in such cases is inconclusive; and to this it is replied, that induction, as *reasoning*, is inconclusive. It is a source of truth different from reasoning; of first truths, the bases of reasonings, as Aristotle has remarked.

April 15.—On the Mathematical Exposition of some Doctrines of Political Economy. By the Master of Trinity.

The object of this paper was to solve algebraically certain pro-

blems which have been solved by Mr. J. S. Mill and others by means of numbers, taken as examples; the principles of these writers being taken for granted in the algebraical solution. Mr. Mill has rightly observed, that instead of saying that prices are determined by the *ratio* of demand and supply, we ought to say that they are determined by the *equation* of demand and supply. This equation may be thus stated. Let p be the price, and q the quantity bought and sold at that price. When p becomes p' , let q become q' ; and p' being equal to $p(1+n)$, let $p'q'=pq(1+mn)$: this is the equation of demand and supply. For different commodities, we have different values. There are such classes of commodities as these: (A.) *Conventional necessities*, for which $m=1$: of these the same quantity is bought whatever be the price. (B.) *Articles of fixed expenditure*, for which $m=0$: on these the same sum is always expended, a smaller quantity being bought in proportion as they are dearer. (C.) *Common necessities*, in which m is between 1 and 0: in these, when the price falls, the consumption is increased, but the money expended diminished. (D.) *Popular luxuries*, in which m is negative: in these, when the price falls, the consumption is so much increased that the money expended on them is increased also. For corn, the mean value of m seems to be about $\frac{1}{2}$: on this supposition a failure of one-fourth in the supply would double the price. The quantity m measures the susceptibility of the price to change when the supply changes, and also the intensity of the demand.

Another division of commodities is, according to the cost of production. These are (α) commodities of fixed and limited supply; (β) commodities of fixed cost; (γ) commodities of increasing cost for increasing supply, as for instance corn in a given limited district. The equation of price for the last case was given.

The like methods were applied to solve certain problems concerning international trade, treated by Mr. Mill. If the relative value of two commodities, C and D, in England and Germany be different, there will be a saving in exporting each from where it is cheaper to where it is dearer; and the question is, at what point prices will settle. We must introduce here the principle of the *uniformity of international prices*; namely, that when the trade is established, the relative prices of C and D will be the same in the two countries: the principle of the *equality of imports and exports* in each country; and the *equation of demand and supply* already stated. By combining these principles, the problem of the resulting price is solved. But it is found that there is no solution possible (that is, no solution in which both countries gain by the trade), except the *mutual demand* for the interchange of commodities be nearly equal. This limitation of the solution is given by the algebraical method, and seems to have been overlooked by previous writers.

The same methods were extended to a greater number of exported and imported commodities; and finally, it was remarked that these calculations are all founded on principles of equilibrium, whereas a state of equilibrium is never attained; and thus the theory

may be very imperfectly applicable, like the equilibrium theory of the tides.

Second Memoir on the Intrinsic Equation of Curves. By the Master of Trinity.

The intrinsic equation of curves, according to which any curve is expressed by means of an equation between its length (s) and its angle of deflection (ϕ), may be conveniently used for many purposes. When a curve is so represented, the portion of the length which comes after a cusp must necessarily be taken as negative. This had appeared anomalous to some mathematicians, on the ground that a cusp is in all cases the limit of a loop. To clear up this point, the author adduces two cases. (1.) The curve of which the equation is $s = a\phi + b \sin \phi$, which is a looped curve when b is less than a , and a cusped curve otherwise. But in this curve it appears that a loop arises from the vanishing of *two* cusps, and of the intervening negative portion of the arc. (2.) The case of the ordinary trochoid, which is a looped curve when the describing point is exterior to the rolling circle, and becomes a cusped curve (a cycloid) when the point is in the circle. But in this case the length of the trochoid is equal to the length of an elliptical arc, which, in the case of the cycloid, coincides with the major axis, and becomes negative beyond the vertex of the ellipse. Other equations were examined, which give *running pattern* curves with cusps, cusped curves with infinite diverging spirals at the extremities, and sinuous curves with infinite converging spirals at the extremities; and certain integrals which occurred in the former memoir on this subject were discussed.

LXVI. Intelligence and Miscellaneous Articles.

THE FIRST IDEA OF THE ELECTRIC TELEGRAPH.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Sidmouth, Nov. 12, 1850.

IN the Number of the Philosophical Magazine for May, I observe that Professor Maunoir claims for his friend Dr. Odier the first idea of the electric telegraph. I herewith send you a translation from a German work by Schwenter, entitled *Deliciæ Physico-Mathematicæ*, and published in 1636, from which it will appear that the *crude* idea of the electric telegraph was entertained upwards of a century before the period alluded to by Professor Maunoir. Indeed Ørsted's grand discovery was alone wanting to perfect the telegraph in 1636. The idea, in fact, appears to have been entertained prior even to this date, for Schwenter himself quotes from a *previous* author.

I am, Gentlemen,

Respectfully yours,

N. S. HEINEKEN.

"How two people might communicate with each other at a distance by means of the magnetic needle:—

"If Claudius were at Paris and Johannes at Rome, and one wished

to convey some information to the other, each must be provided with a magnetic needle so strongly touched with the magnet, that it may be able to move the other from Rome to Paris. Now suppose that Johannes and Claudius had each a compass divided into an alphabet according to the number of the letters, and always communicated with each other at six o'clock in the evening; then (after the needle had turned round $3\frac{1}{2}$ times from the sign which Claudius had given to Johannes) if Claudius wished to say to Johannes, 'Come to me,' he might make his needle stand still, or move till it came to *c*, then to *o*, then to *m*, and so forth. If now the needle of Johannes' compass moved at the same time to the same letters, he could easily write down the words of Claudius and understand his meaning. This is a pretty invention; but I do not believe a magnet of such power could be found in the world."

Quoted from "*the Author*" by Schwenter, in his *Deliciæ Physico-Mathematicæ*, p. 346. 1636.

ON THE SULPHURIC AND NITRIC COMPOUNDS OF BENZIN AND NAPHTHALIN. BY M. A. LAURENT.

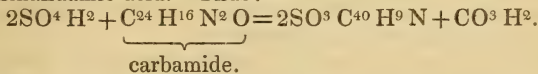
The author states that the last memoir of M. Piria on sulphonaphthalidamic acid induces him to publish the labours which he has undertaken on the same subject, but employing different processes.

He has considered nitrobenzide, anilin, nitronaphthalin, as derived by substitution from benzin and naphthalin, and that they ought to possess greater or less analogy with them.

It is well known that by treating the greater number of them with sulphuric acid, there are obtained sulphobenzidic, sulphanilic, sulphonaphthalic, and nitrated sulphonaphthalic acids.

These acids may also be obtained by other processes. Thus, by treating sulphonaphthalic acid by nitric acid, nitrated sulphonaphthalic acid is formed, $\text{SO}^3, \text{C}^{10} \text{H}^7 \text{X}$.

If this latter be put into contact with sulphuret of ammonium, sulphonaphthalidamic acid is obtained, $\text{SO}^3, \text{C}^{10} \text{H}^7 \text{Ad}$. This may also be obtained by means of naphthalidamic carbamide; it is to be gently heated with concentrated sulphuric acid; carbonic acid is immediately disengaged, and the liquor, diluted with water, deposits sulphonaphthalidamic acid. Thus:—



By prolonging the action of the nitric acid on the sulphonaphthalic acid binitrated sulphonaphthalic acid is obtained, the ammoniacal salt of which, crystallized in fine yellow needles, contains $\text{SO}^3 \text{C}^{10} \text{H}^6 \text{X}^2 + \text{H}^3 \text{N}$. This salt, treated with sulphuretted hydrogen, deposits sulphur, and a new nitrated acid is formed, which appears to be nitrated sulphonaphthalidamic acid, $\text{SO}^3 \text{C}^{10} \text{H}^6 \text{X Ad}$.

Although nitrated naphthalin is unknown, M. Laurent is of opinion that this base is formed by treating binitrated naphthalin with

sulphuretted hydrogen, for in conducting this operation, he obtained a carmine-red alkali, which fused by heat in close vessels.

When benzin is successively treated with nitric acid, sulphuretted hydrogen and sulphuric acid, sulphanilic acid is obtained; and it may also be procured in the following manner:—boil sulphobenzidic acid with nitric acid, and a new acid is obtained, the ammoniacal salt of which consists of $\text{SO}^3 \text{C}^6 \text{H}^5 \text{X} + \text{H}^3 \text{N}$; it is therefore nitrated sulphobenzidate of ammonia; by treating this with sulphuretted hydrogen, sulphanilate of ammonia is formed.

On pouring a little nitric acid into nitrated phthalate of ammonia, a salt is deposited, which contains $\text{C}^8 \text{H}^5 \text{XO}^4 + \text{H}^3 \text{N} + 2\text{Aq}$. By heating this salt till it begins to fuse, it loses water, and is converted into nitrated phthalimide, containing $\text{C}^8 \text{H}^4 \text{XNO}^2$.—*Comptes Rendus*, Octobre 14, 1850.

ON THE DISTILLATION OF MERCURY BY HIGH PRESSURE STEAM. BY M. VIOLETTE.

This new process for the distillation of mercury, consists in immersing the mass to be distilled in a current of the vapour of water, heated from 350° to 400° Centigrade: the vapour acts at once as the heating agent and mechanical agent; it first heats the metal so as to produce distillation, and then drives before it and draws the mercurial vapour, the reproduction of which it facilitates; it hastens the distillation, just as a hot current of air increases the evaporation of water; the aqueous vapours, charged with mercurial vapour, are condensed together in a common refrigeratory; the metal separates at the bottom of the receiver, while the condensed water occupies the upper part. It is curious to observe the liquid thread which flows from the refrigeratory; two currents or threads are distinguishable, an upper one which is water and below is the mercurial thread; there is a continuous current of both. No bumping occurs, and the operation goes on as quietly and as easily as the distillation of water.

The apparatus employed by the author in these experiments, consists of,—1st, a cast iron cylindrical retort, receiving the vessel which contains the mercury; 2ndly, an iron worm, which, being heated, the vapour of water circulates in it, and being heated to a proper degree, enters the retort, traverses it from one end to the other, the mercury being immersed in it; it then escapes, with the mercurial vapour, and both are condensed in a refrigeratory.

The author gives in a series of tables the results which he has obtained by a series of experiments relating to the distillation of mercury, both alone and amalgamated; he states the quantity of vapour necessary, and the economical advantages of the new process, which he thus details:—

1. *Facility of the Operation*.—Simple ebullition and the distillation of water are substituted for the difficult and dangerous distillation of mercury; in which there is more trouble in managing the fire, more danger of breakage of the apparatus, more difficulty in removing the metal, more wear of the retort; whereas in the new

process the temperature is constant and fixed, and much lower than the red heat usually employed.

2. *Economy of Operation*.—One workman alone can manage an apparatus charged with 1000 kilogrammes of amalgam; the new process is adapted even to larger dimensions.

3. *Economy of Fuel* is certain, and practice alone can state the amount of it; no useless expenditure of fuel will occur, since the heat employed will not be greater than required for the distillation of the metal.

4. *Economy of Mercury*.—The distillation of 100 kilogrammes of silver amalgam occasions the loss of two kilogrammes of mercury. There are produced and annually distilled six millions of amalgamated silver; there is therefore a loss of 120,000 kilogrammes of mercury, worth at least one million of francs, which loss the new process avoids.

5. *Public Health*.—In the new process there is no loss of mercury; the mercurial vapour is condensed with the vapour of water; further, in the common operation, mercurial vapour fills the whole of the apparatus, and when it is opened at the close of the operation, the vapour is diffused in the atmosphere, whereas in the new process the vapour has driven all metallic vapour from the apparatus, and there is no danger in opening it. Thus the operation is complete, and the employment of high pressure steam seems to have effected the long-sought solution of the problem, of perfectly preserving the workmen from the mortal attacks of mercury in the numerous and important uses in which this metal is distilled.—*Comptes Rendus*, Octobre 14, 1850.

ON THE PRESENCE OF SUCCINIC ACID IN THE HUMAN BODY.

BY M. W. HEINTZ.

On examining the colourless liquid contained in hydatid cysts, which are frequently developed in the liver, and sometimes even in the muscles, the author discovered that this liquid contains appreciable quantities of succinate of soda.

The liquid contained in the hydatid cysts, extracted from the liver of a woman, was colourless; it contained the remains of hydatids, which rendered the liquid turbid when it was shaken. Its density was 1.0076. It was alkaline, and contained mere traces of albumen. Neither sulphuric acid nor phosphoric acid was detected in it, but much chloride of sodium, and a little lime, potash and magnesia. In order to extract from this liquor the organic substances which it contained, M. Heintz evaporated it to dryness; during the evaporation pellicles were formed, which were renewed on removal. The residue of the evaporation, containing crystals of common salt, was mixed with alcohol, which occasioned the separation of a thick syrup; the alcoholic solution did not appear to contain urea, creatin, or uric acid; when evaporated to a syrupy consistence, it at first deposited crystals of common salt, and afterwards some feathery crystals, which were purified by dissolving in a small quantity of

water, and precipitation from it by concentrated alcohol. The aqueous solution of these crystals, treated with hydrochloric acid, deposited small crystals of an organic acid, slightly soluble in cold water. On attentively examining these crystals, M. Heintz ascertained that they were identical with succinic acid; the experiments first performed were merely qualitative, but they were afterwards completed by the elementary analysis, of a small quantity certainly, of these crystals. The form of the crystals was, moreover, that of an oblique rhombic prism; and M. Heintz found, by very exact ad-measurement, that the angles of these crystals were equal to those of pure succinic acid. These experiments sufficiently demonstrate the interesting fact, announced by the author, relative to the existence of succinic acid in the animal œconomy.—*Journ. de Pharm. et de Chim.*, Septembre 1850.

ON A NEW COMPOUND OF SULPHUR, CHLORINE AND OXYGEN.

BY M. MILLON.

To prepare this, fill a bottle of the capacity of 4 to 5 litres with moist chlorine, and introduce at first 20 to 30 grms. of chloride of sulphur saturated with chlorine, and then 2 or 3 grms. of water; agitate the mixture, and surround the bottle with a freezing mixture of ice and common salt during 4 or 5 hours. A great disengagement of hydrochloric acid occurs; the bottle is to be again filled with moist chlorine and replaced in the freezing mixture, and this is to be repeated till the chloride of sulphur becomes a crystalline mass. These crystals are destroyed with violent action by water, alcohol or diluted acids. But if, after freeing them from the chloride of sulphur which renders them impure, they are allowed to fall into a very dry tube, the open end of which is closed by the lamp, these crystals in two or three months soften, and in eight months become an extremely fluid liquid; it is an isomeric transformation which becomes evident, not only by the change of its physical qualities, but also by the alteration of its chemical properties. Thus the liquid is no longer, as when in the crystallized state, decomposed by water, with violence, alcohol or weaker acids; on the contrary, when poured into water it is quietly deposited at the bottom of the vessel in the form of an oil, which eventually changes completely into sulphuric and sulphurous acids. This transformation is entirely in agreement with its analysis, which leads to the composition of sulphur, chlorine and oxygen, $S^2 O^3 Cl^2$:—

S^2	400	or 25·20
O^3	300	.. 19·93
Cl^2	886·4	.. 55·87
	1586·4	100·00

The analysis is performed with the liquid modification; a weighed vessel is filled with it, and is broken in a bottle containing nitrous nitric acid. The reaction which occurs is moderated by cooling the bottle; the sulphur is subsequently estimated in the state of sulphate

of barytes, and the chlorine in that of chloride of silver.—*Journ. de Pharm. et de Chim.*, Août 1850.

PRÉPARATION AND ANALYSIS OF CODEIA.

BY DR. ANDERSON.

This alkali was obtained, as usual, from the mother-liquor from which morphia had been precipitated by ammonia. As the codeia forms only a sixteenth to a thirtieth of the morphia, it is of course mixed in this fluid with a corresponding quantity of muriate of ammonia, which must be decomposed by potash in order to obtain it. Much advantage is gained, however, by first evaporating the fluid to crystallization, and expressing the crystals deposited, as in this way the greater part of the muriate of ammonia, which is the more soluble salt of the two, is left in solution; and by repeating the crystallization many times, it may be entirely removed, and crystals obtained which are pure hydrochlorate of codeia. For the preparation of codeia, however, it would be worse than useless to carry the process thus far, as the solubility of hydrochlorate of codeia and ammonia differs so little, that much of the former salt would be lost; but by carrying it a certain length, the greater part of the sal-ammoniac may be separated without any material loss of codeia, and the subsequent steps of the process much facilitated. The crystals so obtained being dissolved in boiling water, strong solution of caustic potash is added in excess, when codeia is in part precipitated as an oil, which by-and-by concretes into a solid mass, and is partly deposited in crystals as the solution cools. By evaporating the fluid, another crop of crystals is obtained; and finally, when the mother-liquor has been concentrated to a very small bulk, it becomes filled on cooling with long silky needles of morphia, which has been retained in solution by the excess of potash. A certain quantity of morphia appears always to remain in solution along with the codeia, but its quantity seems to vary considerably. Its presence in this solution has been observed before, and it has been stated that it exists in the form of a double salt with codeia; Dr. Anderson states, however, that this is not consistent with his experience; at least the salt separated from the muriate of ammonia by successive crystallizations contained no morphia, but, as has been already stated, was pure hydrochlorate of codeia.

The crystals of codeia precipitated by potash in the manner described, are always more or less coloured. They are purified by solution in hydrochloric acid, boiling with animal charcoal, and reprecipitation with a slight excess of potash, and the precipitate obtained finally dissolved in æther to separate any morphia which may adhere to it. For this purpose hydrous æther is best adapted; and it ought to be free from alcohol, as if any be present, the æther evaporates and a syrupy fluid is left behind, which refuses to crystallize. When the æther is anhydrous, it dissolves the codeia with much greater difficulty; and by evaporation small crystals are deposited, which are anhydrous.

Dr. Anderson submitted the codeia to analysis, and deduced from his results the formula $C^{36} H^{21} NO^6$, giving in 100 parts—

Carbon	72·24
Hydrogen	7·02
Nitrogen	4·68
Oxygen	16·06
	100·00

which agrees with the previous determination of Regnault.

Codeia crystallized from water or from hydrous æther is obtained in crystals, often of considerable size, belonging to the right prismatic system, and presenting a considerable number of modifications. These crystals contain two equivalents, or 5·67 per cent. of water.

Codeia is an extremely powerful base, rapidly restoring the blue of reddened litmus, and precipitating oxides of lead, iron, cobalt, nickel, and other metals from their solutions. It is precipitated by potash from its salts, and is generally stated to be insoluble in that alkali; but this is true only of very highly concentrated solutions, as a considerable quantity of strong potash may be added to a saturated solution of codeia in water without producing precipitation; and even when a large amount of potash is added, a certain quantity of the base is still retained in solution. Codeia is soluble in ammonia, but not more so than in water: 100 parts of a moderately strong solution of ammonia dissolved, at 60°, 1·46 part of codeia; and according to Robiquet, 100 parts of water, at 59°, dissolve 1·26 parts. Contrary to what is usually stated, Dr. Anderson found that codeia is precipitated from all its salts by ammonia; it does not, however, fall immediately, but is slowly deposited in small transparent crystals.—*Trans. Royal Soc. Edinburgh*, 1850.

ON HYPOCHLOROUS ACID AND THE CHLORIDES OF SULPHUR.

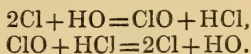
BY M. E. MILLON.

The author states, that when an aqueous solution of chlorine is kept in bottles not exposed to light, it is preserved without any appreciable change; but if this solution be exposed for some time to the direct influence of the solar rays, peculiar reactions occur. Thus chloride of lead is thereby converted into pure oxide, and the chloride of manganese gives a black precipitate of peroxide; whereas fresh solution of chlorine does not in any way modify these two chlorides.

In examining to what combination of chlorine this characteristic oxidizement of the chlorides of lead and manganese belonged, M. Millon found that it resided exclusively in the hypochlorous acid. This acid, ClO , may thus be recognized when contained in a solution of chlorine, even in very small quantity; for these two reagents, and especially that of manganese, are extremely sensible.

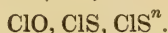
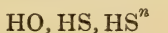
This action of chlorine on water is evidently identical with that of chlorine on the greater number of hydrogenated substances; water is substituted for hydrogen; and if the phenomenon is limited, it is

because the hydrochloric acid in its turn destroys the hypochlorous acid, and regenerates chlorine: this arrangement will be seen by reversing the two members of the equation,



The hydrochloric and hypochlorous acids can coexist only in the presence of a large quantity of water; the author proved this by direct experiment.

M. Millon is of opinion, that the very simple molecular relation existing between water and hypochlorous acid ought to be extended to the chloride of sulphur, which belongs to this system of sulphuretted hydrogen, of which chlorine has replaced the hydrogen, equivalent for equivalent. It will be seen by this arrangement, that the most chlorinated chloride of sulphur will contain two equal equivalents of chlorine and of sulphur. Thus it will be in vain, as chemists well know, to attempt to combine more than one equivalent of chlorine with one equivalent of sulphur; on the contrary, it is easy to combine several equivalents of sulphur with one equivalent of chlorine. The polysulphurets of hydrogen then come under consideration, and the following symmetries result:—



Ann. de Chim. et de Phys., Août 1850.

ON THE DISCOLORATION OF SILVER BY BOILED EGGS. BY M. GOBLEY.

It is observed by the author as a well known fact, that silver, when brought into contact with eggs which have been heated, is blackened; this discoloration is owing to sulphuret of silver. It is usually admitted that this sulphuret is formed by the action of the sulphuretted oils supposed to exist in the yolk of the egg; not having found in this body anything of this nature, M. Gobley thought it would be interesting to examine into the cause of this phenomenon.

Yolk of egg at common temperatures and when gently heated, does not discolour silver, even by contact of several hours' duration.

Albumen as procured from the egg, does not tarnish silver, but when the temperature is raised, it gives it a brown tint, which is stronger as the heat is greater.

The discoloration of the silver is then due to the sulphur contained in the albumen, and not to that supposed to exist in the oil of the egg; but is the sulphuret formed under these circumstances, the result of the immediate action of the sulphur upon the silver, or by the application of heat do the sulphur and the alkali of the albuminous matter react upon each other, so as to form a substance which is afterwards decomposed by this metal?

The following experiments induced the author to adopt the second of these opinions:—

1. Paper impregnated with lead, when exposed to the vapour disengaged from the heated albumen, is not sensibly discoloured.

2. Coagulated white of egg, treated with dilute sulphuric acid, disengages a gas which sensibly discolours lead paper.

3. Coagulated white of egg, brought into contact with a solution of acetate of lead, becomes of a light brownish tint.

The yolk of egg, under the circumstances above described, does not act upon the salts of lead.

Thus the discoloration of silver, by the action of eggs which have been heated, is owing to the reaction of the sulphuret, formed by the combination of the sulphur of the albumen on the soda which it contains.—*Journ. de Chém. et de Pharm.*, Novembre 1850.

ON A TEST FOR PROTEIN COMPOUNDS. BY M. E. MILLON.

The very acid solution which is obtained by dissolving mercury in its weight of nitric acid containing $4\frac{1}{2}$ equivalents of water, is an extremely sensible reagent for all albuminoid substances, and for a considerable number of secondary products which are connected with it.

This nitromercurial solution communicates a red colour of considerable intensity to these several substances, and it is easy thus to ascertain the presence of 1-10,000dth of albumen, and even a smaller quantity.

To give a direct idea of the delicacy of this reagent, and perhaps of the advantage which may be taken of it in studying the organism of vegetables, the author states that cotton, various kinds of starch, and gum-arabic, when put into contact with it, assume a very distinct rose tint. Urines are almost immediately coloured rose-red; after the nitromercurial solution has been added to them, and the mixture having been heated, the urea is destroyed.

The albumen of the blood and of vegetables, fibrin, casein, gluten, legumin, silk, wool, feathers, horn, the epidermis, gelatin, chondrin, protein, crystallin, the cornea, &c., are rendered of a more or less intense red colour by this solution.

When protein becomes soluble by the prolonged action of alkaline solutions, or by that of sulphuric acid, the same colour is always produced, but no insoluble matter is obtained; the solution becomes of a deep red colour, without yielding any precipitate.

Xanthoproteic acid, the chlorites of protein, and the oxides of protein derived from them, separate from the preceding products; they are not at all coloured red. This reagent exhibits differences which it is very interesting to examine. The author has already ascertained, that, by the action of chlorine upon albumen till the gas ceases to be absorbed, there are formed no less than three substances very distinct from each other.

The nitromercurial solution is prepared by adding to the metal an equal weight of nitric acid, containing $4\frac{1}{2}$ equivalents of water; reaction takes place rapidly in the cold; when it has become moderate, the solution is to be very gently heated till the metal is completely dissolved; at this point there are to be added to one volume of the mercurial solution two volumes of water. After some hours the liquid portion is to be decanted from the crystals of nitrate and nitrite of mercury. This solution reacts in the cold on albuminoid

substances ; but the reaction is not complete below 60° to 70° Cent. ; it is even proper to boil the mixture. Long-continued contact of excess of the reagent does not alter the red matter. The author has kept for more than twelve months strongly reddened albumen in contact with excess of the mercurial solution.

It is to be observed that the reacting power does not reside either in the proto- or pernitate of mercury, nor even in a mixture of them. It is requisite that the solution which contains these two salts should have nitrous acid added to it ; till this is done, no colour is produced. Pure pernitate of mercury, saturated with nitrous acid, reacts sensibly, but much less than the mixture of the proto- and persalts saturated with the same nitrous acid. Thus the simplest method of preparing this solution consists in treating mercury with nitric acid in the method already described.—*Ann. de Chim. et de Phys.*, Août 1850.

METEOROLOGICAL OBSERVATIONS FOR OCT. 1850.

Chiswick.—October 1. Fine: cloudy: clear. 2. Very fine. 3. Slight rain. 4, 5. Foggy: very fine. 6. Foggy: very fine: rain. 7. Boisterous. 8. Clear: fine. 9. Slight fog: very fine. 10. Clear: very fine: rain. 11. Windy: stormy showers: clear. 12. Clear: very fine. 13. Overcast. 14, 15. Exceedingly fine: sharp frost at night. 16. Clear and fine. 17. Very fine. 18. Foggy: very fine. 19. Overcast: fine. 20. Fine. 21. Fine: clear and cold. 22. Clear: dense clouds: overcast. 23. Heavy rain. 24. Densely overcast: rain. 25. Cloudy. 26. Clear: cloudy and fine: clear and frosty at night. 27. Clear: fine: rain. 28. Rain: fine: clear. 29. Clear and fine: sharp frost. 30. Frosty: rain: clear. 31. Overcast: fine.

Mean temperature of the month 44°·32

Mean temperature of Oct. 1849 49 ·55

Mean temperature of Oct. for the last twenty-four years . 50 ·51

Average amount of rain in Oct. 2·67 inches.

Boston.—Oct. 1, 2. Fine. 3, 4. Cloudy. 5. Fine. 6. Fine: rain P.M. 7. Cloudy: rain early A.M. 8. Cloudy. 9, 10. Fine: rain P.M. 11. Cloudy: rain A.M. and P.M. 12. Fine. 13, 14. Cloudy. 15, 16. Fine. 17. Cloudy. 18. Fine. 19, 20. Cloudy. 21. Cloudy: rain early A.M. 22. Cloudy. 23. Fine: rain A.M. and P.M. 24—26. Cloudy: rain A.M. and P.M. 27. Fine: rain P.M. 28. Fine: rain early A.M. 29. Fine. 30. Cloudy. 31. Cloudy: rain early A.M.

Applegarth Manse, Dumfries-shire.—Oct. 1. Fair, but unsettled-looking. 2. Fair, but dull and cloudy. 3. Drizzling greater part of the day. 4. Heavy showers P.M. 5. Fog A.M.: hail: rain P.M. 6. Fog A.M.: heavy rain P.M. 7. High wind: heavy rain. 8. Fair, but cloudy. 9. Slight hail: light rain P.M. 10. Fair and frosty: shower P.M. 11. Fair and cold. 12. Fair and cold: frost A.M. 13. Moist and drizzly. 14. Slight showers. 15. Frost: high wind P.M. 16. Rainy, but slightly so. 17. Slight showers: cleared P.M. 18. Cloudy all day. 19. Slight showers. 20. Clear and fine. 21. Frost A.M.: shower P.M. 22. Frost severe: rain P.M. 23. Frost still: shower. 24. Frost severe. 25. Raw: dull: slight shower. 26. Frost hard: fair all day. 27. Frost very hard: thermometer 27°: shower P.M. 28. Thaw: fine: clear. 29. Frost again: fine and clear. 30. Rain A.M.: moist all day. 31. Fair and fine throughout.

Mean temperature of the month 44°·2

Mean temperature of Oct. 1849 44 ·0

Mean temperature of Oct. for the last twenty-eight years ... 46 ·0

Average rain in Oct. for twenty-three years 3·50 inches.

Sandwich Manse, Orkney.—Oct. 1. Clear: dry: aurora. 2, 3. Fine: aurora. 4. Fine. 5. Fine: showers: aurora. 6. Fine: solar halo. 7. Rain. 8. Cloudy: showers. 9. Showers: drying. 10. Showers: sleet-showers. 11. Bright: fine. 12, 13. Drizzle: showers. 14. Showers: sleet-showers. 15. Bright: rain. 16. Rain. 17. Rain: cloudy. 18. Rain. 19. Bright: cloudy. 20. Bright: clear. 21. Clear. 22. Cloudy: rain. 23. Showers. 24. Showers: clear. 25. Clear: showers. 26. Clear: frost: fine: aurora. 27. Cloudy: rain: aurora. 28. Bright: clear: aurora. 29. Sleet-showers: fine: aurora. 30. Rain: showers. 31. Bright.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfriesshire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.

[illegible]

FIG. 1.



FIG. 2.

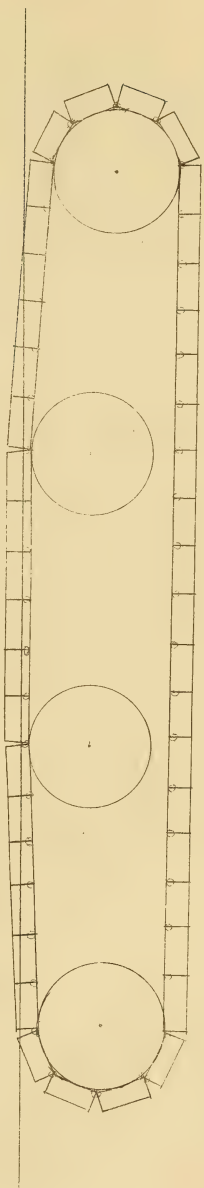


FIG. 3.



FIG. 4.



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LXVII. *On Aloine, the Crystalline Cathartic Principle of Barbadoes Aloes.* By JOHN STENHOUSE, LL.D., F.R.S.L. & E.*

ABOUT two months ago I received from my friend Mr. Thomas Smith, apothecary, Edinburgh, a quantity of a brownish-yellow crystalline substance which he had obtained from Barbadoes aloes. Mr. Smith's process consisted in pounding the previously dried aloes with a quantity of sand, so as to prevent its agglutinating, macerating the mass repeatedly with cold water, and then concentrating the liquors *in vacuo* to the consistence of a syrup. On remaining at rest in a cool place for two or three days, the concentrated extract became filled with a mass of small granular crystals of a brownish-yellow colour. This is the crude substance to which Mr. Smith has given the name of aloine, and which appears to constitute the cathartic principle of aloes. The brownish-yellow crystals obtained in this way are contaminated with a greenish-brown substance, which changes to brownish-black on exposure to the air, and still more rapidly when it is boiled. In order to purify the crystals of aloine, therefore, they must first be dried by pressure between folds of blotting-paper, and then repeatedly crystallized out of hot water till they have only a pale sulphur-yellow colour. The aqueous solutions of aloine must on no account be boiled, but simply heated to about 150° F., as at 212° F. aloine is rapidly oxidized and decomposed. By dissolving the purified crystals of aloine in hot spirits of wine, they are deposited, on the cooling of the solution, in small prismatic needles arranged in stars. When these crystals have a pale yellow colour, which does not change when they are dried in the air, they may be regarded as pure aloine.

Aloine is quite neutral to test-paper. Its taste is at first sweetish, but soon becomes intensely bitter. Aloine is not very soluble either in cold water or in cold spirits of wine; but if the water or the spirits of wine are even slightly warmed,

* Communicated by the Author.

the solubility of the aloine is exceedingly increased: the colour of these solutions is pale yellow. Aloine is also very readily dissolved by the carbonated and caustic fixed alkalies in the cold, forming a deep orange-yellow solution, which rapidly grows darker, owing to the oxidation which ensues. The effects of ammonia and its carbonate are precisely similar. When aloine is boiled either with alkalies or strong acids, it is rapidly changed into dark brown resins. A solution of bleaching-powder likewise gives aloine a deep orange colour, which soon changes to dark brown. Aloine produces no precipitate in solutions either of corrosive sublimate, nitrate of silver, or neutral acetate of lead. It also yields no precipitate with a dilute solution of subacetate of lead; but in a concentrated solution it throws down a deep yellow precipitate, which is pretty soluble in cold water, and is therefore difficult to wash. This precipitate is by no means very stable; and when it is exposed even for a short time to the air, it becomes brown.

When powdered aloine is thrown, in small quantities at a time, into cold fuming nitric acid, it dissolves without evolving any nitrous fumes, and forms a brownish-red solution. On adding a large quantity of sulphuric acid, a yellow precipitate falls, which, when it is washed with water to remove all adhering acid and then dried, explodes when it is heated. It plainly, therefore, contains combined nitric acid. I could not, however, succeed in obtaining this compound in a crystalline state, as when it was dissolved in spirits, it appeared to be decomposed. When aloine is digested for some time with strong nitric acid, much nitrous gas is evolved, and it is converted into chrysammic acid, but without the formation of any nitropicric acid, as is always the case when crude aloes is subjected to a similar treatment. A quantity of aloine was boiled with a mixture of chlorate of potash and muriatic acid. The acid solution was evaporated to dryness, and digested with strong spirits of wine. The greater portion of the spirits was removed by distillation; and the remainder, when left to spontaneous evaporation, yielded a syrup which could not be made to crystallize. Not a trace of chloranil was produced.

When aloine is destructively distilled, it yields a volatile oil of a somewhat aromatic odour, and also a good deal of resinous matter. When aloine is heated on platinum foil it melts, and then catches fire, burning with a bright yellow flame, and emitting much smoke. It leaves a somewhat difficultly combustible charcoal, which, when strongly heated, entirely disappears, not a trace of ashes being left.

A quantity of aloine dried *in vacuo* was analysed with chromate of lead in the usual way.

I. 0.2615 grm. aloine gave 0.5695 carbonic acid and 0.14 water.

II. 0.2415 grm. aloine gave 0.5250 carbonic acid and 0.126 water.

	Hydrated aloine.		Found numbers.	
	Calculated numbers.		I.	II.
34 C .	2550.0	59.47	59.39	59.24
19 H .	237.5	5.54	5.97	5.79
15 O .	1500.0	35.09	34.64	34.97
	4287.5	100.00	100.00	100.00

The formula derivable from these analyses is $C^{34}H^{19}O^{15}$, which, as we shall presently see, is $= C^{34}H^{18}O^{14} + HO$, or aloine with one equivalent of water.

The aloine which had been dried *in vacuo* was next heated in the water-bath for five or six hours, and was also subjected to analysis.

I. 0.251 grm. aloine dried at 212° F. gave 0.550 carbonic acid and 0.128 water.

II. 0.2535 grm. aloine dried at 212° F. gave 0.564 carbonic acid and 0.129 water.

III. 0.234 grm. aloine dried at 212° F. gave 0.521 carbonic acid and 0.114 water.

	Calculated numbers.		I.	II.	III.
34 C .	2550	61.07	60.51	60.67	60.72
18 H .	225	5.39	5.66	5.65	5.42
14 O .	1400	33.54	33.83	33.68	33.86
	4175	100.00	100.00	100.00	100.00

The aloine employed in these analyses was prepared at three different times. These results give $C^{34}H^{18}O^{14}$ as the formula of anhydrous aloine, that dried *in vacuo* being a hydrate with one equivalent of water.

When the aloine was allowed to remain in the water-bath for more than six hours, it continued slowly to lose weight, apparently owing to its undergoing partial decomposition by the formation of a brownish resin. The loss of weight gradually continued for a week or more, but became very rapid when the aloine was heated to 302° F., when it melted, forming a dark brownish mass, which when cooled became as hard and brittle as colophonium. It still, however, contained a good deal of unaltered aloine, as I ascertained by crystallizing it out with hot spirits and analysing it. Much of the aloine, however, had been changed, most probably by oxidation, into a dark brown uncrystallizable resin.

Brom-aloine.—When an excess of bromine is poured into a

cold aqueous solution of aloine, a bright yellow precipitate is immediately produced, the amount of which increases on standing, while at the same time the supernatant liquid becomes very acid from containing free hydrobromic acid. The precipitate, after it has been washed with cold water to remove adhering acid, is dissolved in hot spirits of wine; and on the cooling of the solution it is deposited in bright yellow needles radiating from centres, which attach themselves to the bottom and sides of the containing vessel.

The crystals of brom-aloine are considerably broader than those of aloine, and have a richer yellow colour and a higher lustre. Brom-aloine is quite neutral to test-paper, is not so soluble in either cold water or cold spirits of wine as aloine, but dissolves very readily in hot spirits of wine.

I. 0.421 grm. substance dried *in vacuo* gave 0.547 carbonic acid and 0.103 water.

0.856 grm. gave 0.848 bromide of silver = 42.16 Br.

II. 0.300 grm. substance gave 0.391 carbonic acid and 0.078 water.

0.661 grm. substance gave 0.649 bromide of silver = 0.2762 Br = 41.78 per cent.

		Calculated numbers.		I.	II.
34 C	. . .	2550.00	35.73	35.43	35.53
15 H	. . .	187.50	2.62	2.71	2.86
14 O	. . .	1400.00	19.63	19.70	19.83
3 Br	. . .	2998.89	42.02	42.16	41.78
		7136.39	100.00	100.00	100.00

The brom-aloine employed in these analyses was prepared at two different times. It is plain therefore from these results, that this bromine compound is aloine, $C^{34}H^{18}O^{14}$, in which 3 eqivs. of hydrogen are replaced by 3 eqivs. of bromine. The formula of brom-aloine therefore is $C^{34}H^{15}O^{14}Br^3$.

When a stream of chlorine gas was sent for a considerable time through a cold aqueous solution of aloine, a deep yellow precipitate was produced. It contained a great deal of combined chlorine; but as it could not be made to crystallize, it was not subjected to analysis. In the present instance, and in those of several other feeble organic principles, such as orcine, chlorine appears to act somewhat too strongly, so that the constitution of the substance is destroyed, and merely uncrystallizable resins are produced. Bromine, on the other hand, is much more gentle in its operations, and usually simply replaces a moderate amount of the hydrogen in the substance, so that, as in the case of orcine and aloine, crystalline compounds are produced.

It has long been known to medical practitioners, that the aqueous extract of aloes is by far the most active preparation of that drug. The reason of this is now very plain, as the concentrated extract of aloes obtained by exhausting aloes with cold water consists chiefly of aloine, by much the larger portion of the resin being left undissolved. Mr. Smith informs me, that, from a series of pretty extensive trials, from 2 to 4 grs. of aloine have been found more effective than from 10 to 15 grs. of ordinary aloes. Aloine is, I should think, therefore likely ere long to supersede, at least to a considerable extent, the administration of crude aloes.

I endeavoured to obtain aloine by operating on considerable quantities of Barbadoes, Cape and Socotrine aloes. These were macerated in cold water, and the aqueous solutions obtained were concentrated to the state of thin extracts on the water-bath. I was quite unsuccessful in every instance. The impurities contained in the extracts in these different kinds of aloes appear, when in contact with the oxygen of the air, to act upon the aloine so as effectually to prevent it from crystallizing. Aloine can only therefore be obtained in a crystalline state by concentrating the cold aqueous solution of aloes *in vacuo*; though, after the aloine has once been crystallized, and it is freed from the presence of those impurities which appear to act so injuriously upon it, the aloine may be quite readily crystallized out of its aqueous solutions in the open air.

Though aloine has as yet only been obtained from Barbadoes aloes, I have scarcely any doubt that it also exists both in Cape and Socotrine aloes. The amount of aloine in Cape aloes is however, in all probability, much smaller than in either of the other two species; for Cape aloes is well known to be a much feebler cathartic, and to contain a mass of impurities. In corroboration of this opinion, I would refer to the fact already mentioned in a previous part of this paper, viz. that when aloine is digested with nitric acid, it is converted into Dr. Schunck's chrysammic acid. Now it has been satisfactorily ascertained that all the three species of aloes yield chrysammic acid, of which in fact they are the only known sources. Cape aloes, as might have been expected, yields by far the smallest amount of chrysammic acid, together with much oxalic and some nitropicric acids. There appears therefore great reason to believe that all the three kinds of aloes contain aloine.

Since the above was written, I have learned from Mr. Smith that he has not succeeded in obtaining crystallized aloine from either Cape or Socotrine aloes. Mr. Smith does not doubt

that both of these species of aloes also contain aloine, though most probably contaminated with so much resin, or some other substances, as prevents it from crystallizing. What tends to confirm Mr. Smith in this opinion is the observation he has made, that when the crude crystals of aloine are allowed to remain in contact with the mother-liquor of the Barbadoes aloes, they disappear and become uncrystallizable. I have also observed a similar occurrence in the mother-liquors of tolerably pure aloine. These become always darker and darker; so that if we continue to dissolve new quantities of aloine in them, at length scarcely any of it crystallizes out, and the whole becomes changed into a dark-coloured magma.

In the year 1846, M. E. Robiquet published an account of an examination he had made of Socotrine aloes. By treating the concentrated aqueous solution of this species of aloes with basic acetate of lead, he obtained a brownish yellow precipitate, which was collected on a filter and washed with hot water. On decomposing this lead compound with sulphuretted hydrogen and evaporating the solution to dryness, he obtained an almost colourless varnish, consisting of a scaly mass, which was not in the least degree crystalline. M. Robiquet subjected this substance, which he called aloetine, to analysis, and obtained the following result:—

	per cent.
8 C	= 27·7
14 H	= 10·8
10 O	= 61·5
	100·0

It is plain therefore that M. E. Robiquet's aloetine, if it really is a definite organic principle, which I very much question, is certainly a very different substance from the aloine which has formed the subject of the present notice.

Glasgow, Nov. 7, 1850.

LXVIII. *On Striated and Polished Rocks and "Roches Moutonnées" in the Lake District of Westmoreland.* By JAMES BRYCE, jun., M.A., F.G.S.*

[With a Plate.]

I. *Introduction.*

THE lake district of Westmoreland and Cumberland has received a very large share of attention from geologists. Its rock-formations have been fully and accurately described,

* Communicated by the Author.

chiefly in several able memoirs by Professor Sedgwick; and respecting the more striking erratic phænomena, such as the dispersion of the Shap granite, there has been much discussion. This subject, indeed, has elicited several papers of the highest order of geological investigation from the pen of Mr. Hopkins of Cambridge; but no detailed examination, that I am aware of, has been made of the superficial formations generally, with the view to determine, as respects the other rocks, either the direction or the æra of the transport on the different sides of the central mountains. Scratched rocks afford evidence on this subject of a most satisfactory kind. Up till this time, however, they have not been noticed in the lake district. It is hoped, therefore, that the present communication will prove not unacceptable to geologists, as a slight contribution towards a better understanding of the erratic phænomena of the lake mountains.

The credit of this interesting discovery is due to Edward Wakefield, Esq. of Birklands, near Kendal. Knowing that in the cuttings of the Kendal and Windermere railway, as well as in quarries adjoining, the surface of the rocks had been laid bare in many places, Mr. Wakefield instituted a careful search along the line in May last, and was so fortunate as to discover two extremely well-marked examples of scratched rocks. While on a visit in Kendal in July I was shown these cases by Mr. Wakefield: in company with him several others were afterwards noticed. I gave a short account of these rocks to the Geological Section of the British Association at the Edinburgh meeting in August, and propose now to describe them in greater detail.

II. *Markings at Jacob Wood.*

The best marked case occurs on the north-west side of Jacob Wood, about one mile south of the Staveley station of the Kendal and Windermere railway, about fifty yards from the railway on the north-east side. It will be most conveniently visited by stopping at the Staveley station, and then proceeding towards Kendal along the turnpike road. Here, on the northern side of a gently rising, inconsiderable undulation of the ground, near to many projecting rough slate rocks, the scratched surfaces are seen. By the removal of a covering of till, containing boulders, several feet in depth, a surface of rock was laid bare extending 53 feet 3 inches from north-east to south-west by 15 feet 9 inches in a direction perpendicular to this. It was uncovered about two years ago in making a road into the adjoining fields; the rock was to have been removed, but it was found so difficult to work, from its great

toughness and hardness, that the quarry was abandoned; and to this circumstance we owe the preservation of perhaps the most perfect specimen of striated rocks to be seen in the three kingdoms. I have visited most of the localities in Scotland where such rocks occur, but have met with nothing so perfect. It is impossible by any drawing or description to convey an adequate notion of the strikingly beautiful and perfect character of the striation, polishing, and rounding of the rocks here exhibited. The accompanying Plate (Pl. II.) representing a portion of the surface will assist. It is from a drawing most kindly taken at my request by James Henry Gough, Esq. of Kendal, and transmitted for the illustration of my paper laid before the Edinburgh meeting already referred to.

The surface is traversed by four eminences of an elliptic form, running in the direction of the breadth, smooth, rounded and highly polished, and presenting exactly the character of the most perfect specimens of the "*roches moutonnées*," so well-described and figured by Agassiz in his work on the Alpine glaciers. These eminences, and the hollows between, are thickly covered in every part by striæ and grooves, the great majority of which run in the direction of 10° W. of magnetic north, or 34° W. of true north, taking the variation at 24° W., which I am informed by Prof. Phillips of York, is its present amount in this district. *They thus point very nearly in the direction of the opening of Kentmere*, or more exactly in the direction of Staveley Head, a hill of no great elevation (1000 feet?) on the western side of the entrance to Kentmere.

Most of the striæ are very fine, being mere touches as by a delicate graving-tool; others are traced with greater boldness; and the grooves are often of considerable width, as from $\frac{1}{4}$ inch to $1\frac{1}{2}$ inch, or perhaps more in a few cases. A great many of the striæ are inclined to the prevailing direction already given; but the angles seldom exceed a few degrees, and by far the greater number deviate very little from the direction above stated. This direction is *across* the beds, or at right angles to the stratification of the slate, which here belongs to the division called the Lower Ludlow Rock by Prof. Sedgwick. It is plain, indeed, that the striæ have no connexion with, or dependence on, the internal structure, or the unequal wearing of component laminæ having different powers of resisting the causes of waste. From the very compact character of the Lower Ludlow Rock, in many places it is often difficult to determine the position of the true beds. "This can sometimes be done," says Professor Sedgwick, "by help of alternating bands of coarser materials wherein the original bedding has not been obliterated by the slaty struc-

ture; a mass of slate between two such bands must have its bedding parallel to them. . . . In many cases we can ascertain the lines of the true beds by an internal and secure test. The planes of the slates are often marked by parallel stripes of different colours. Among the finer green slates these stripes are generally paler than the other parts of the rock; and as they mark the original lines of sediment, they are therefore parallel to the true bedding; indeed they generally mark the passage from one bed to another. Sometimes these stripes are seen on slaty laminæ, cutting through pyritous bands with shells and corals; and in such cases the stripes upon the smooth surfaces of the slates are always parallel to the fossil bands*."

Now, though in the case we are considering the bedding is very obscure, we have for our guidance "the internal and secure test" above mentioned, the true direction of the beds being marked by parallel stripes of different colours. These facts are stated thus fully, because there are still some who maintain that rock-striation is structural, or in the direction of the beds, and has been produced by the unequal wearing of the constituent laminæ under the action of water, and is not due to the passage of ice or boulders across the surface. But it is unnecessary to enter into a discussion of views so entirely opposed to established facts†.

III. *At Birthwaite Station.*

The next example is seen at Birthwaite, the present terminus of the same railway, within the grounds belonging to the station. Here the surface of the Lower Ludlow Rock has been stripped of its covering of detritus to an extent of 45 feet in a direction perpendicular to the striæ, and of 15 feet in a line with them. This surface is similarly divided into several rounded, smooth, and polished bosses or eminences of an elliptic form, whose greater axis is in the direction of the striæ, or across the surface; and which, though less regular in form, have the same "*moutonnée*" character as at Jacob Wood. The prevailing direction of the striæ and grooves is magnetic N. and S., or about 24° W. of true N. Many, however, are inclined to this direction at various angles. On the western side the direction is chiefly N.W. and S.E., but in the middle and on the eastern side they have the range above stated,

* Third letter addressed to Mr. Wordsworth in Hudson's Guide to the Lakes, one of the most interesting and valuable books of this class which we have ever met with.

† See Prof. Oldham's Address to Geol. Soc. Dublin, 1850, p. 27, and the Journal of the Geol. Soc., vol. ii.

which is very nearly coincident with the direction of the upper part of Windermere and the Vale of Ambleside. As in the former case, the striæ and grooves run across the stratification of the rock, which is very hard and tough. The strata are nearly on edge, and range about E.N.E. and W.S.W.

IV. *At Birthwaite Church.*

The inclosures of the new church of Birthwaite, about a quarter of a mile further west on the road to Ambleside, afford another remarkable instance. Slate rocks having the same range and inclination here rise above the ground in many detached knolls, the surfaces of which in most parts are traversed by numerous striæ and furrows running very nearly in the direction of the magnetic meridian, as in the instance last given. Some are, however, inclined to this direction, usually at small angles. A few only were noticed, running about N.E. and S.W. These markings are seen not only upon horizontal or slightly inclined surfaces, but are marked horizontally even upon those which *are vertical or slightly overhanging*. Beyond the inclosure, on the western side, the rocks are similarly marked.

There is another series of peculiar markings in this locality apparently of posterior date and different origin. These are deeper, and have their edges less worn and rounded, while they are not so much discoloured by weathering as the rest of the rock; and thus they look like artificial furrows made by an iron instrument passing, under great pressure, in parallel lines across the surface. They are numerous and regular, and constant in their direction, which is the same in general as that of the other grooves and striæ; they are horizontal upon vertical surfaces; and hence, as well as from the height of the rock above the surface of the ground, it is difficult to conceive that they have been produced otherwise than by natural causes. I visited the place in company with Edward Wakefield, Esq. of Birklands, and John Elliot Howard, Esq. of Tottenham, near London; and though we made a lengthened and careful examination of the phænomena, we felt uncertain what conclusion to deduce, and were all of opinion that, till further evidence was obtained, it would be unsafe to ascribe the whole of these markings to the operation of natural causes.

V. *Detached Markings.*

On the rising ground east of Birthwaite Church, a rocky surface of small extent exhibits striæ and grooves running in the same direction, but less perfect than those already described.

Again, about half a mile west of Staveley, on the south side of the Ambleside road, upon the upper surface of a quarry which slopes to the north, very perfect furrows and striæ are seen running about S.E., and the surface is rounded and polished. A portion of an elliptic boss was noticed, showing that before the rock was quarried so far, there had existed a highly illustrative example of the "*roche moutonnée.*" By the further removal of the superficial detritus, similar markings will no doubt come into view.

VI. *Theory of the Markings.*

Pursuing this inquiry in company with the gentlemen before mentioned, the Ambleside and Grasmere vales were traversed as far as the summit of Raise Gap; and the upper part of the valley running thence northwards as far as the head of Thirlmere, with the whole western flank of Helvellyn, without noticing further cases. Our examination was, however, rather cursory; a closer investigation would probably have detected additional examples. It is highly desirable that the valleys descending to the N.E. and N.W., and opening towards Penrith and Cockermouth, should be carefully examined with reference to the existence of such markings. Their discovery in these valleys, in connexion with the facts stated in this communication, would throw much light upon the history of the superficial deposits of the lake district, and the nature of the forces by which the great masses of transported materials filling the outer valleys and the plains at the base of these mountains were brought into their present situations. In a series of most able papers, supported by mathematical reasonings and calculations, Mr. Hopkins has undertaken to show that the lake mountains have been raised to their present height by many successive elevations of a paroxysmal character; that these gave rise to great "waves of translation" and diverging currents of enormous power; so great, for example, "that an elevation of 100 or 150 feet would produce a current capable of transporting, for at least a short distance and under favourable circumstances, a block of immense weight," and consequently lighter materials to great distances*. During the periods of repose the action of the sea would scoop out valleys near the centre of elevation, and hence the great system of diverging valleys which run from the central group. This operation would be determined in the first instance, and greatly facilitated in its progress by the prior ex-

* Trans. Camb. Phil. Soc. vol. viii. part 2; Journal Geol. Soc. vol. iv. p. 70. See also a valuable paper by Prof. Whewell, Journ. Geol. Soc., vol. iii. p. 227.

istence of those great faults, which are known to traverse most of the valleys radiating from the central group; for example, in Long Sleddale, Kentmere, the upper part of Windermere, Coniston, the Duddon, Eskdale, Wastwater, Ennerdale, Mardale, &c. The materials thus loosened would be transported from their parent beds, further and further at each successive elevation of the land, and so might reach great distances, as the hills of Derbyshire and the eastern wolds of Yorkshire. During this period the striation and furrowing may have been produced upon the surfaces exposed, below the sea-level, to the wearing action of the drift; and it is worthy of note, that the phænomena now detailed respecting these confirm, so far as they go, the theory we have been stating. The grooves and striæ have a diverging direction, those at Jacob Wood having a more *easterly* bearing than those at Birthwaite; while the valley of the Kent presents to us a succession of ridges of sand and gravel of coincident direction with the former, and containing fragments of rocks which must have come from the north. The hill immediately adjoining Kendal, on which Kendal Castle stands, another hill south of it, of the same form and dimensions, and many others in this vale, might be cited as examples. It would be highly interesting to ascertain, by an examination of the kind before alluded to, whether this partial conformity is an isolated phænomenon, or is part of a great system of diverging striæ, marking the *quaquaversal* direction of the denuding and transporting forces.

The phænomena which we have described are such as glaciers could have produced, and are actually giving rise to in many places; and it is not denied that glaciers may have once existed in the high central valleys. But their agency, or that of icebergs, is quite insufficient, as has been shown by Mr. Hopkins, "to account for the most important phænomena of distant transport from the Cumbrian mountains;" such, for example, as the dispersion of the Shap boulders; and as two causes for the same effect are inadmissible, it seems unnecessary to have recourse to the agency of ice at all, since the diluvial theory, as above stated, is adequate to explain all the appearances—the deposition of the superficial detritus, and the distant transport of boulders, as well as the scooping out of the valleys, and the striation and grooving, the rounding and polishing of exposed rocky surfaces.

High School of Glasgow,
October 22, 1850.

LXIX. *Analysis of the Theory of Equations. Second and Concluding Part.* By JAMES COCKLE, Esq., M.A., Barrister-at-Law. In a Letter to T. S. DAVIES, Esq., F.R.S. &c.*

[The First Part will be found at pp. 351-367 of vol. xxxii. S. 3.]

2 Pump Court, Temple,
October 8, 1850.

MY DEAR SIR,

1. **V**ARIOUS considerations, which it is not necessary to detail, render it desirable that, in resuming and concluding the subject of a former letter of mine to you, I should confine my remarks to *rational* equations. And I shall be forgiven for reminding you, and those under whose notice this letter may perhaps eventually come, of the existence in print, not only of that former letter, but also of my Notes on the Theory of Algebraic Equations, commenced in the forty-sixth volume of the *Mechanics' Magazine*, and the Third and Concluding Series of which is now in the course of publication in that Journal. I mention these Notes rather than other papers of mine, because in them, as in this Analysis, I have attempted, though from different points of view, to take something like a general survey of the whole subject.

2. All those equations, the discussion of which forms the primary object of the Theory before us, involve at least one general symbol—that of the unknown quantity—and are, consequently, algebraic in their form. But a little consideration enables us to perceive that such equations may be advantageously separated into two great classes, the characteristics of which are respectively furnished by an observation of the nature of the *coefficients* of the unknown quantity. When any one or more of the coefficients of a given equation is a letter, or general symbol, the equation may be termed a *literal*, symbolic, or algebraic equation. When all its coefficients are numbers, it may be called a *numerical* or arithmetical equation. These two classes then, (*a*) the literal, and (*b*) the numerical, include all equations algebraic in their *form*.

3. But, if we should carry our views further than the mere form of the equation, and look beyond that to the object we have in view in discussing it, we must admit a third and hybrid class which partakes of the natures of the other two, being literal in its form, but virtually numerical, inasmuch as all our processes treat the literal coefficients, not as general symbols, but as generalized numbers. For the sake of clear and accurate classification, I shall separate the class of literal equations into two others, the algebraic, and the quasi-algebraic. In

* Communicated by T. S. Davies, Esq., F.R.S.L. & E., F.S.A.

justification of the use of this last term, I may observe that Sir J. W. Lubbock has applied the term *quasi-literal* to certain astronomic developments (Theory of the Moon, Preface to Part V., p. ix.).

4. We thus have three great classes of equations,—(1) the algebraic, (2) the quasi-algebraic, and (3) the numeric. And this is no arbitrary classification, but one arising from the very nature of the subject; for, call these classes by what name we will, the results aimed at in the first are very different from those anticipated in the second and third, which last, again, have different aims and processes. The theory of *algebraic* equations belongs exclusively to Symbolical Algebra, and rigorous symbolical results are the only ones aimed at. In the *quasi-algebraic* theory we investigate such *general* processes, and properties of equations, as shall be ancillary to the operations of the *numeric* theory, and shall enable us to apply the latter operations with certainty and effect.

5. Adopting substantially the arrangement given in my former letter, we shall have the whole Theory of Equations comprised in seven departments, of which the *algebraic* theory contains four;—viz. I. *Elimination* and Solution; II. Transformation; III. Multiplicity of Solutions; IV. Relations between Coefficients and Roots, including the theory of *Symmetric Functions*. In the *quasi-algebraic* theory are comprised two departments, viz. V. The nature of the roots; VI. Their limits. To the *numeric* theory appertains VII. The numerical values of the real roots and of the real quantities involved both in the real and imaginary parts of the expressions for the unreal roots.

6. Hence, neither the quasi-algebraic nor the numeric theory belong exclusively to Arithmetical Algebra. It is conceivable that an unreal root may be completely determined by purely numerical processes; and, in fact, Dr. Rutherford has, in his 'Complete Solution of Numerical Equations,' entered upon such a course of inquiry. To this I may add that you yourself, in some notes printed with my former letter, evidently regard the determination of the unreal roots as coming within the scope of the numeric theory. In like manner, the quasi-algebraic theory contemplates the existence of unreal roots, and seeks to ascertain their nature and limits. As my purpose is, however, not to discuss the last-mentioned theories, but to proceed to that of equations purely algebraic, I must dismiss the subject—at least for the present. But, before I do so, I cannot refrain from naming a most important addition to the quasi-algebraic theory, in the shape of a tract on 'The Analysis of Numerical Equations,' which constitutes the First

Part of a projected series of papers 'On the General Principles of Analysis' by our friend Professor J. R. Young, late of Belfast. This first Part is the only one that has as yet appeared. The obstacles to the further publication of the work are stated on the cover of Part I., and will, I sincerely trust, be speedily obviated by its more extended circulation among mathematicians. It will richly repay perusal, and gives new and efficient means of analysing an equation. In explanation of the principle involved in it, I shall venture to say, that, some two or three years ago, in the course of my *Horæ Algebraicæ*, I employed Mr. Gompertz's process for porismatizing expressions, and I porismatized the surd factors of a quadratic for the purpose of ascertaining whether in certain cases, the roots of the given quadratic being *known*, the surd factors so porismatized admitted of being made to vanish when the natural and obvious signs were given to the radicals. [See *Mechanics' Magazine*, vols. xlvii. pp. 409, 410, and xlviii. pp. 181-183.] By reversing this process, and changing its object, Professor Young has been enabled, from the examination of the porismatized surd factors of equations of all degrees, to arrive at a knowledge of the nature and limits of the unknown roots. This statement neither implies, nor is it intended to imply the slightest claim to Professor Young's researches. I neither have, nor do I wish to be considered as having, *any*. This appears to be the proper place for adding, that some of the topics included in the algebraic theory—such as elimination and transformation—enter likewise into the other theories in certain cases. The numerical process of solution is in fact, in certain cases, a series of transformations. And in this, as in most other similar instances, we find one department insensibly encroaching upon another. For example, it is difficult to refrain from considering the algebraic solution of a cubic as it bears, both upon the nature, and upon the numerical evolution of the roots. Such considerations are the common ground upon which two or more departments coalesce, but it will serve no purpose to allude further to them.

7. A basis for the classification of algebraic equations is presented to us, either in the *dimensions* to which the unknown quantities enter into a given equation, or in the *number* of those unknowns, or in a combination of considerations derived from both sources. An equation of n dimensions is said to be of the n th *degree*. An equation between n unknowns is said to be of the n th *order*. I have now for some time adopted this word *order* in exclusive reference to the number of unknowns contained in an equation. And I shall continue to

apply it rigidly in the same sense. Great practical convenience will be found to result from this nomenclature; but I will observe that, as equations of the first four degrees are designated by the respective names of linear, quadratic, cubic, and biquadratic equations, so I have proposed to give to equations of the first four *orders* the respective titles of simple, binary, tertiary, (or ternary,) and quaternary equations. This will save much circumlocution, but it will be necessary to banish the term simple as applied to equations of the first *degree*. No inconvenience need attend the new application of the term. The term 'simple equation,' as hitherto used, is a mere synonym for 'linear equation,' while, as applied to equations of the first degree, the latter term is far more appropriate, on the ground of analogy with 'quadratic,' &c. Analogy would further justify us in using the abbreviations '*n*-ic equation' to denote an equation of the *n*th *degree*, and '*m*-ary equation' to denote an equation of the *m*th *order*. The term '*m*-ary *n*-ic' would completely define any algebraic equation. While upon this subject I may take the liberty of referring you to my remarks on it at pp. 509, 510, 582, &c. of vol. xlv. of the *Mechanics' Magazine*.

8. But, of these appellations 'degree' and 'order,' which is to be Genus, and which Species? It is important to make a correct choice, for an improper selection would vitiate our arrangement, and introduce confusion instead of method. My answer to the above inquiry is, that *degree* is to be regarded as Genus, and *order* as Species. And this arrangement will not only be found to be, philosophically speaking, more accurate, but it is also more practically convenient, and, unquestionably, more in conformity with the historic development of the *Theory of Equations*. It was only by reducing its solution, to that of two simultaneous binary equations, that a cubic was first solved. In other words—if we use the word *complex* in contradistinction to simple—the solution of a simple cubic involved the solution of complex equations of degrees virtually, if not formally, lower than a cubic. Hence, in what follows, I shall carry, as far as may seem desirable, the discussions of different orders of equations of a lower degree before proceeding to those of equations of a higher degree. The benefit of this, to a limited extent the ordinary course, will be seen as we proceed.

9. Of simple linear equations little more need be said than that their solution is effected by algebraic addition, subtraction, multiplication and division, or one or more of those operations, all of which we may include in the common name *reduction*. Where several simultaneous complex linear equa-

tions are presented for solution, we proceed by *elimination* to the formation of simple linear equations, each involving one of the unknowns. In such equations each unknown admits of *one* value only, and no elevation of degree is introduced by elimination. If we have n linear relations among powers (or roots) of n unknown quantities, then provided that the same quantity is affected with the same power (or root) in each of the relations, we may determine the values of the powers (or roots) of each of the unknowns by linear elimination, and consequently the unknown itself may be determined by evolution or involution. These two latter terms (which might be comprehended under the common name *volution*) I should propose to restrict to arithmetical or, at all events, to determined quantities. And I should incline to appropriate the word *elevation* to that operation which consists in affixing a positive integral index to an undetermined quantity. It is needless to say what is meant by the *expansion* of a quantity, so elevated, but by *contraction* we may denote the passage from the developed to the undeveloped form of the elevated quantity. By *depression* we may signify the affixing a fractional index with unity for its numerator and a positive integer for its denominator. When the indices are the same as in the previous cases but *negative*, we may term the respective operations negative elevation or depression as the case may be. When the index has for its numerator and denominator integers prime to one another, the operation indicated consists, of course, both of elevation and depression.

10. It is only for the purpose of calling your attention to its connexion with the above considerations respecting complex linear equations, and with the fact that, when a power is known, the root may be considered as known, that I here mention Varignon's solution of a quadratic. This solution (see *Phil. Mag. S. 2. vol. iv. pp. 314, 462, 463*) is an application to quadratics of the indeterminate method employed in the original solution of a cubic. But the ordinary solution affords ample exercise for reflection, independently of its introducing us to the idea of multiple solutions, and the germ of the theory of symmetric functions. It presents to us two distinct methods; the one capable of being rendered perfectly general, the other partially so. The process common to both—that of adding and subtracting the same quantity to an expression, and so changing its form and leaving its value unaltered—may be called the *method of superposition*. In the case of a quadratic, the quantity superposed is (I speak of the ordinary process) the square of half the coefficient of the first power of the unknown. This *superposition* is followed by a

contraction, the contraction by *depression*, and, lastly, a simple transposition furnishes us with the required roots.

11. In the case of a quadratic the superposition is of the simplest kind. A determined quantity is added and subtracted. But, if we suppose the quantity superposed to be undetermined in its value, we give our analysis a greater range. Under this new form I shall term it the *subsidiary method*, as, in my 'Notes' above alluded to, I have previously done, in allusion to the subsidiary quantity so introduced. The subsidiary method is not, however, limited to the use of one new undetermined quantity. Simpson's solution of a biquadratic, as given by Murphy at p. 54 of his *Theory of Equations*, is an instance of a subsidiary process involving three undetermined quantities B, C, D , besides the unknown x . As there employed, the quantity m appears to be used, rather to determine the subsidiaries, than as a subsidiary in itself. But by an alteration of the process, which I shall not dwell on, m might either be dispensed with altogether, or be made a strictly subsidiary quantity, and, thus, the means of observing uniformity of method. So, by a slight change of form, my solution of an imperfect cubic (*Phil. Mag. S. 3. vol. xxii. pp. 502, 503*) may be presented as effected by a quadratic subsidiary function of x and p . Again, Ferrari's solution of a biquadratic is a subsidiary one. And there is a certain form of equations of the fifth degree (that which I proposed for solution in the *Diary* for 1849) which admits of a subsidiary solution.

12. The case, that I have just alluded to, is that in which we superpose a subsidiary quantity or quantities foreign to the expression in its primitive form. But suppose that we have before us a quadratic function involving, in its original state, m undetermined or unknown quantities besides determined or known ones. Then, in general, by means of successive superpositions we may reduce the given quadratic function to the form of a sum of m squares of linear functions of the unknowns, together with the square of a known quantity. This remark I shall bring to bear presently.

13. But, before doing so, let us proceed to examine the species of complex equations that belong to the genus quadratics. The first question that now presents itself is,—Can we solve two simultaneous binary quadratics by means of equations of no higher degrees than quadratics? I call this the *first* question, because it is sufficiently obvious that, if we are required to solve n linear and one quadratic, all of the $(n+1)$ th order, or of some lower one, then no elevation of degree will be introduced by elimination. Nor will such elevation be introduced by any number of linear equations in combination with

others. Hence we may always confine our attention to the non-linear equations, and consider that as done which is always possible, viz. the elimination of as many unknowns as there are linear equations. But, to our question. In general, two binary quadratics do not admit of solution by quadratics and linears only; but such a system may be solved when either the squares or the product of the unknowns appear in every term involving the unknowns. It will shortly be seen that this particular case introduces into the theory of equations a general process of solution.

14. We have now exhausted the subject of the determinate solution of quadratics by means of quadratics, and are about to enter on the exhaustless field of indeterminate quadratics, that is to say, of systems of simultaneous quadratics whose order is greater than the number of equations to be satisfied. For, true to a principle originally laid down, I shall discuss, as far as practicable, the various species of quadratics before going to the higher genera of equations. The first question, of the indeterminate kind, that is presented to us is,—Whether it be possible to solve two simultaneous tertiary quadratics by means of quadratics and linears only? The answer is,—Yes, by various processes, an outline of which I give below.

(a.) They may be solved by a combination of *superposition*, *transformation*, and the process used in solving two binary quadratics free from the first power of the unknowns. Calling this the *homogeneous process*, we may sketch our solution as follows:—By successive superpositions (in the first of which the superposed quantity involves *two*, in the second *one*, and in the third *none*, of the unknowns) we may reduce one of the given quadratics to the form of a sum of four squares, one of which does not involve the unknowns. Take the linear functions of the unknowns of which the squares are obtained by these superpositions as *new* unknowns. Transform the second of the given tertiary quadratics into another in which the new unknowns shall be the only undetermined quantities. As none of the old unknowns have yet been determined, so the new system is as indeterminate as the original one. Now, determine one of the three new unknowns, in terms of the other two, in such a manner as that neither of the three shall enter linearly into the second quadratic. Then the two given tertiaries are reduced to two binaries of a form which, as we have already seen, is completely solvable by quadratics. I term this the *method of homogeneous elimination*, and I have given its application to the solution of two tertiaries under a somewhat different form (see *Mechanics' Magazine*, vol. xlviii. pp. 605, 606), by decomposing each unknown into the sum of two new unknowns.

(b.) If, after reducing one of the two given tertiaries to the form of four squares as above suggested, we group them two and two, and equate each group to zero; then, each group being the sum or difference of two squares, is capable of being decomposed into linear factors. Equate to zero one factor of each group, then the tertiary so grouped vanishes. And, eliminating, by means of the two vanishing factors, two of the unknowns from the remaining tertiary, the latter becomes a simple quadratic in the third unknown, which is then determined; and afterwards the others are obtained by substitution for the third in the vanishing factors, which are then treated as ordinary binary linears. This is the *method of vanishing groups*, of which I have given an "Account" in the *Philosophical Magazine* (S. 3. vol. xxxii. pp. 114-119), and to which, as far as I had then developed it in print, you will find references in a note [†] to my fifth paper on the Transformation of Algebraic Equations, at pp. 178, 179 of the third and concluding volume of the *Mathematician*.

(c.) Although Mr. G. B. Jerrard's *method of vanishing coefficients* (as I have proposed to term it) be not directly applicable to the solution, in their explicit form, of three tertiary quadratics by quadratics and linears only, yet, in all probability, it would not be difficult, by decomposing the unknowns, or some of them, into the sums of others, to give the tertiaries such a form as to admit of the application of Mr. Jerrard's processes. Full information respecting Mr. Jerrard's analysis will be found in his 'Mathematical Researches,' and in Sir W. R. Hamilton's 'Inquiry,' into the subject at pp. 295-348 of the Sixth Report of the British Association. Mr. Jerrard's method of elimination differs from the ordinary one in this,—that from an expression he expels, successively, all the powers of a quantity, which he wishes to remain undetermined in value, by making the *coefficient* of each power vanish. This he does by a powerful *algebraic* indeterminate analysis, quite distinct from the analysis known by that name in the ordinary treatises on algebra. Those who delight to trace the analogies which the different branches of all sciences suggest, may derive satisfaction from comparing Mr. Jerrard's method with that pursued in the strictly Diophantine Analysis,—that given, for instance, at pp. 306-343 of the fourth edition of Professor J. R. Young's *Algebra*. Consider, by way of example, the manner of rendering rational the cube root of a cubic function of x . This rationalization, when possible, is effected by obtaining an equation in x , from which all powers of x , except the first, are expelled.

15. Of course we may readily solve two quaternary quadratics without equations higher than quadratics. For we

may give an arbitrary value to one of the unknowns and then apply, to the resulting tertiaries, the processes just discussed. So, we may make the fourth unknown subservient to a simplification of those processes. If we employ the method of homogeneous elimination, we may (see *Mec. Mag.* vol. xlviii. p. 512) so use the fourth unknown as to avoid the superpositions and transformation prescribed in (a.). If we employ the method of vanishing groups, we may, by expelling the first power of one of the unknowns, avoid one of the superpositions; or, by expelling the first power of all the unknowns simultaneously, we may abridge *all* the superpositions. In applying the method of vanishing coefficients we should also, most probably, materially shorten our operations. And, possibly, in all three methods an advantageous relation might be established between the two given complex quadratics. If we have given for solution two quadratics of the fifth order, Mr. Jerrard's process becomes explicitly and immediately applicable. I would here add, that, by a *combination* of the method of disposable multipliers with that of vanishing coefficients (to which latter method the former may, in innumerable instances, be made subservient), we may, as I have shown (*Mech. Mag.* vol. xlix. p. 10), solve two tertiary quadratics without any decomposition whatever.

16. And here, with another remark or two, I shall leave the subject of the *quadratic solution* of complex quadratics. You will understand, by the term "quadratic solution," solution without the occurrence of equations of a degree higher than the second. You will not, however, fail to remark that the subject is inexhaustible, and that there are infinite numbers of systems which admit of a quadratic solution. The method of vanishing groups, for instance, enables us to obtain quadratic solutions of m simultaneous quadratics of the $(2^m - 1)$ th order. It must be borne in mind, that, in attempting the application of the method of homogeneous elimination to the quadratic solution of more than two complex quadratics, we must, by the aid of superposition, disposable multipliers, and a sufficient number of undetermined quantities, endeavour to form a number of distinct systems, each system consisting of two equations in each of which two unknowns are homogeneously involved; the unknowns being different in each system. Or we may combine, as circumstances may suggest or require, the three different general methods considered in the 14th article of this letter. Let me here add an important remark connected with the method of vanishing groups. When the squares of all the unknowns are absent from a given complex quadratic, or when any square or squares

vanish at a critical stage of our superpositions, the general method admits of modification. I have exhibited a particular, but useful, case of such modification (I allude to the case of tertiary functions) at page 547 of vol. xlvii. of the *Mechanics' Magazine*. In the case where some or all the squares disappear from a tertiary quadratic function, Professor Hearn has (*Mathematician*, vol. iii. p. 198) suggested a process which may also be generalized and adopted in place of my modification. On Professor Hearn's neat and able discussion of surfaces of the second degree I have made short comments at p. 249 of vol. iii. of the *Mathematician*, and also at pp. 44, 45 of the Supplement to that volume, with which (supplement) the existence of the valuable periodical in question terminated.

17. Having sufficiently discussed the quadratic solution, I proceed to the *biquadratic determination* of complex quadratics, by which I mean such a determination of them as shall render their solution possible, without having to satisfy any simple equation of a degree higher than a biquadratic. Now there is one system of three tertiary quadratics, the solution of which is capable of being rendered dependent on that of a simple biquadratic. I mean the case in which all the three given equations are, what I shall call, *pseudo-homogeneous*, that is to say, are free from the linear dimension of x . The system of equations involved in "Colonel Silas Titus's Problem" belongs to such a system. I have given a discussion, and some references relating to this problem, at pp. 34-36 of vol. I. of the *Mechanics' Magazine*. The method by which I have solved three *general* pseudo-homogeneous quadratics (*Mech. Mag.*, vol. xlviii. p. 512) is, in its principle, the same as that by which Frend solved Titus's problem. And Frend's method is an ingenious and philosophical generalization of the process employed in solving two pseudo-homogeneous binary quadratics, unless indeed the latter is a particular case of Frend's. In the form in which I have exhibited the solution of pseudo-homogeneous binary (See *Mech. Mag.*, vol. xlviii. p. 511) and tertiary quadratics, we avoid the introduction of new unknowns.

18. If we had three *general* quadratics of the fifth order, we might effect their biquadratic determination by the method of homogeneous elimination (*Ibid.* p. 606); but then we must decompose each of the unknowns into the sum of two others. The same determination may be effected, without any decomposition, by the method of vanishing groups. Thus, if, by five superpositions, we reduce one of the quadratics to the form of a sum of six squares, we may group these squares two and two, make the three sums vanish, depress, and eliminate three

of the unknowns from the remaining two given quadratics, both of which may be solved by means of a biquadratic and a quadratic. And we may effect the biquadratic determination of three quadratics of the sixth order without any transformation or decomposition of the unknowns, but by the direct application of the method of homogeneous elimination (*Ibid.* p. 512). By a combination of the method of disposable multipliers with that of Mr. Jerrard (similar to the combination alluded to in article 15 of this letter), three 5-ary quadratics admit of a biquadratic determination (*Ibid.* vol. xlix. pp. 10, 11).

19. A system of four quadratics of the eighth order is capable of biquadratic determination, by a combination of superposition, disposable multiplication, and the method of vanishing coefficients. I have pointed out two methods of effecting this determination. [*Ibid.* vol. l. pp. 33, 34. The 'second solution' of the last-mentioned page, however, requires the corrections which I subsequently gave at p. 106 of the same volume.] This system of four 8-ary quadratics is also capable of *cubic* determination (*Ibid.* p. 34, 'Third Solution'). And here I leave the subject of the determination of complex quadratics, throughout which you will have borne in mind the distinction between a determination of *equations*, and that of the variables which they contain. The terms 'quadratic,' &c. prefixed to 'determination,' indicate the sense in which the term determination is used; and it may, at some future time, be desirable to give a still more precise system of nomenclature in reference to that term. By regarding their *determinations* we shall often be able to obtain an advantageous comparison of problems. You will remark that, whenever a system of equations admits of a simple determination of less than five dimensions, such a 'determination' amounts to a solution of the system. In other words, when n is less than 5, a simple n -ic determination of the system gives us the means of solving it.

20. Of simple cubics I shall say nothing. When complex quadratics and a complex cubic are presented to us simultaneously for solution, it will always be possible to solve them, provided that the system would admit of *quadratic solution*, if in place of the cubic we had a quadratic. For, if no elevation of degree be introduced into a quadratic by elimination, then none would be introduced were a cubic substituted for it. Linear equations in fact introduce no elevation, whatever be the degree of the equation in which the elimination takes place. And I may here observe—once for all—that, whenever a system of quadratics admits of a general quadratic solution, then we may substitute for one of the quadratics an

equation of any degree that we please, and the system will always admit of a determination of the degree of the equation so substituted.

21. Denoting, in conformity with a preceding nomenclature, by the term *pseudo-homogeneous equation* of the n th degree, an equation in which the unknowns enter to the same (n) dimensions in every term excepting the absolute (determined or known) term, there are two cases, besides those before enumerated, in which pseudo-homogeneous equations admit of solution. Those cases, like the others, are, where the pseudo-homogeneous equations are two in number, both of the same dimensions, and those dimensions less than 5; that is to say, where they are both cubics, or both biquadratics. All that we can say of pseudo-homogeneous equations of the fifth, or other higher degree, is, that they admit of a *determination* of the fifth or other respective degree. I have noticed (Mech. Mag. vol. xlviii. pp. 538, 539) other properties of pseudo-homogeneous equations, not necessary to be now mentioned, and I have indicated (Ibid. pp. 606, 607) a process by which two general cubics, if they be of the ninth order, may be reduced to a system of binary pseudo-homogeneous cubics. And I have also intimated, what might be shown without much difficulty, that two complex general biquadratics, if of an order sufficiently high, might be reduced to a pseudo-homogeneous binary system, and so resolved by means of simple biquadratics. If, in the discussion of questions such as these, we wish to employ the method of homogeneous elimination in as pure a form as possible, we must, as I have already intimated, arrange our equations in systems of *pairs* of biquadratics or cubics, and pairs or *triplets* of quadratics, availing ourselves of the methods of disposable multipliers and of indeterminates to effect such arrangement, if indeed it be possible. The solution of two 9-ary cubics, alluded to above, is sketched out by means, not simply of the method of homogeneous elimination, but also of the methods of vanishing groups and vanishing coefficients, one or other of them.

22. Upon the subject of *simple biquadratics* I shall address but few words to you, first observing that I have made some remarks upon the subject at pp. 105, 106 of vol. iii. of the Cambridge Mathematical Journal, where you will find the same extension given to the solution of Descartes that Simpson gave to that of Ferrari. I once thought that I had obtained formulæ for the reduction of biquadratics to a binomial form (Phil. Mag. S. 3. vol. xxviii. pp. 132, 133), but I subsequently (Ibid. 395) corrected my erroneous inference, the origin of

which I made clear in a second and somewhat more detailed explanation (*Ibid.* vol. xxxii. pp. 51, 52, Art. II.). And I have given some developments on the subject of the pp. 132, 133 (just cited) at pp. 32-34 of the Supplement to vol. iii. of the *Mathematician*.

23. Although invalid for effecting the transformation of biquadratics to a binomial form, the investigations, referred to in the last paragraph, enable us to transform the general equation of the fifth degree to a trinomial form. [See *Phil. Mag.* S. 3. vol. xxviii. pp. 132, 133, 395, with the details and improvements comprised in article I. to III. of pp. 50-52 of vol. xxxii.] And my trinomial transformation of the equation of the fifth degree appears to possess two advantages over that of Mr. Jerrard, whose genius first discovered the transformation in question;—(1) we only employ three unknowns, and, (2), the symmetric functions of the roots of the original equation, which in Mr. Jerrard's process (*Researches*, p. 77 *et seq.*) rise as high as the *thirtieth* degree inclusive, in mine are only involved as far as the *twentieth*.

24. In the problem which we have just been considering, we see that the solution of the equation of the lower degree and higher order precedes, and renders possible, the transformation of the higher degree and lower order, affording a confirmation of the accuracy of our system of classification. And this indeed would be, in all likelihood, much further confirmed had I space to do full justice to all the various questions discussed or suggested by Mr. Jerrard and Sir W. R. Hamilton. The necessity, however, of observing some limits prevents me at present from doing much more than refer to my own labours, with a deep appreciation of, and interest in, those of others. Hence I here only allude to my own attempted solution of the general equation of the fifth degree (see *Phil. Mag.* S. 3. vol. xxvii. pp. 125, 126, 292, 293, vol. xxviii. pp. 190, 191), the failure of which I afterwards (*Ib.* vol. xxviii. p. 395) pointed out, and, subsequently (see par IV. pp. 52, 53, vol. xxxii.), more clearly explained.

25. In Sir W. R. Hamilton's discussion (referred to above) of Mr. Jerrard's process, certain limits are given within which the application of the method is confined. I have sought (see *Phil. Mag.* S. 3. vol. xxix. pp. 181-183, vol. xxx. pp. 28-30) to give a similar limitation to the processes of the method of vanishing groups. But the results which I have arrived at are not altogether satisfactory, and I should wish that any judgement, formed on the subject, may be suspended until I shall have been enabled to give a complete discussion of the question, or, until some one, who may take a sufficient interest

in the theory of equations to be induced to follow up the investigation, shall have prosecuted the subject, to such an extent as to be enabled to pronounce a decisive opinion upon the question of the limits of the last-named method.

26. The subject of equations has, however, lately been regarded with an apathy which is, to say the least, extremely singular, when we consider its vast range, its importance, and the interest which it undoubtedly possesses in itself. The powers of that great indeterminate process, to the advance of which the labours of Mr. Jerrard have given a fresh impetus, remain as yet untried in practice, and its resources unfathomed even by those who, from its exhaustless springs, might draw the means of irrigating fields of science heretofore deemed barren. The indeterminate methods, when considered in reference to the theory of equations, strictly so called, have either completely changed, or presented us with the means of so changing, the whole face of the science. The indeterminate theory is, if I may use the expression, one of the Senses of Algebra.

27. But, even already, the mind has, presented to it, another object than that of merely threading the endless pathways of the indeterminate theory, and of glancing down its illimitable vistas. The indeterminate methods must be compared with one another as *wholes*, and their mutual relations thoroughly investigated. Of the three general methods, which I have here adverted to, I am disposed to think that the substantial limits are the same. Thus, two simultaneous tertiary quadratics, which are *immediately* solvable by the method of vanishing groups, may be rendered amenable to the other methods (of vanishing coefficients, and of homogeneous elimination,) by means of transformation or decomposition. Is the method of vanishing groups then the simplest form of indeterminate process? Or is there an altogether different method, of which the three named above are particular forms, to which they (and all others that may be discovered) can be reduced, and in which all others are included? To answer this question, definitely and satisfactorily, would be to do, for the indeterminate theory, that which Lagrange did for the ordinary theory of equations, when he showed the ultimate identity of all the apparently isolated methods, then known, of solving equations of the first four degrees, and the common basis upon which they all rest.

28. You will forgive me, if I suggest a route to this general indeterminate method, and, to this end, coin a term, *adlimination*, to express an operation by which a quantity is *introduced* into any system of equations. Adlimination and Elimination

nation will be converse terms, denoting inverse operations. Now, let us consider two general tertiary quadratics. Eliminate one unknown between them, and we have a binary biquadratic; and, if I mistake not, we may so determine one of the unknowns, as to reduce this biquadratic to a simple biquadratic, of such a form as to be capable of solution by means of quadratics only. Regard as known the process of adlimination by which we pass, from the binary biquadratic, to the system of two tertiary quadratics. So that, in point of fact, if we had been asked,—What must be the *order* of two quadratics in order that they may admit of *quadratic* solution?—we might answer thus. Find the least number of indeterminates by which the elevated equation (a biquadratic), which is the result of ordinary elimination, may be depressed to a quadratic; and to this add unity, and as many quantities as may be necessary to form the original binary system by adlimination. If it were required to find the order of two cubics capable of cubic solution, I should proceed thus. Find how many indeterminates are required in order that an equation of the ninth degree may be depressed to a cubic. To this number add unity and the quantities required for adlimination. Although you may find this paragraph at present somewhat vague, I do not think that it will eventually turn out to be meaningless. More on the subject I shall not say now.

29. The apathy of which I have complained above is not limited to the present time. When, generalizing Descartes' method of representing curves, Pacent, John Bernouilli, and Clairaut had represented surfaces by an equation between *three* variables, and when, moreover, the general reduction of the equation of curves of the second degree had been effected, all the materials were at hand for effecting a transformation which is among the most striking that Mr. Jerrard has presented to algebra—that of the equation of the fifth degree to a trinomial form. And, even now, this general reduction, as effected by vanishing groups, is the simplest way of performing the transformation in question. But the step was never taken until the transformation had been effected by other means. Had the general reduction of a tertiary quadratic presented itself in its full force to earlier investigators, it would have given them a *primâ facie*, but illusory, transformation of a biquadratic to a binomial form, and would have involved them in a fallacy that the algebra of that time might not readily have exposed. But, attention once drawn to the point, we might now have been surveying a richly cultivated garden, instead of what is almost a barren waste. And yet we must not complain that those mighty minds were idle,

although we may perhaps regret that this particular subject did not possess greater interest to them.

30. I do not intend to trouble you about the limits of *transformation* of equations. I shall only say that possibly, nay, *probably*, the limits of the application of *all* indeterminate processes are the same as those imposed by Sir W. R. Hamilton on Mr. Jerrard's method. But I must be allowed to add a word with respect to the failure of Mr. Jerrard's solution of the equation of the fifth degree by means of indeterminates, and also with respect to the failure of my own solution. It must not be thought that either of those solutions involve any error of *principle*. On the contrary, the *principles* involved in both of them may be advantageously employed in the solution of many problems. The failures in question arise from the root of the general equation being, in both instances, presented in the shape of a vanishing fraction. In both cases the ball is well-directed, but it wants the impetus required to carry it home; in neither is the arrow wrongly aimed, but it shivers against an invisible barrier interposed between it and its object.

31. I may here remind you that, in the third and final volume of the *Mathematician*, I have given three papers on the Theory of Symmetric Functions. These papers are chiefly devoted to the application of Mr. Jerrard's theory to the subject of what I have (*Phil. Mag. S. 3. vol. xxviii. p. 191, and elsewhere*) termed *critical* functions. In the third paper (*Math., pp. 30, 31 of the Supplement to vol. iii.*) I have accounted, *a priori*, for the occurrence of these critical functions. It might perhaps be desirable to give a further discussion of what we may call the *semi-critical* functions; that is to say, of functions into which P enters to less dimensions than those of the function. The occurrence of these critical and semi-critical functions must be narrowly watched. Their occurrence will often introduce vanishing fractions into expressions where we should little have anticipated their occurrence. While upon the subject of symmetric functions, I may add that I think there is great weight in a remark at the conclusion of Mr. Stevenson's treatise on equations (2nd edit.). I have given a specimen of such equations, as Mr. Stevenson probably had in view when he penned the conclusion of that work, in the *Philosophical Magazine* (*S. 3. vol. xxvii. p. 294*).

32. I still propose to adhere to the same use of the terms *direct* and *indirect*, when applied to solutions of equations, as that which I adopted in my former letter to you on the present subject. But I would notice that Euler's method is at the same time a direct and an indirect one. It is direct so

far as its own processes are concerned, but, in the form in which it is usually exhibited, an assumption is made that we are acquainted with the roots of a *cubic*, and with the manner in which those roots enter into the coefficients. I have some time since (see *Phil. Mag.* S. 3. vol. xxxii. pp. 421, 422) given one or two corrections and additions to my former letter; but I must here remark that Vandermonde should share with Lagrange the glory of the discovery of a general analysis of equations, and that to Lagrange is due a mode of representing the roots of equations, which some have attributed to the late distinguished Murphy. Connected with, and presenting some general resemblance to, the methods of Lagrange and Vandermonde, is my *method of symmetric products* respecting which you will find full information in my "Notes on the Theory of Algebraic Equations." The connexion and resemblance consists in this; that the linear functions whose *product*, in my method, is *symmetric*, are the same as those whose *squares*, *cubes*, and higher powers have, in the methods of Lagrange and Vandermonde, only *one*, *two*, &c. values respectively. I should wish those "Notes" to be read in connexion with this letter.

I remain, my dear Sir,

Yours most truly,

To T. S. Davies, Esq., F.R.S.L. & E. JAMES COCKLE.
&c. &c. &c., Woolwich.

Postscript.—October 25, 1850. Let me add—

(33.) That, besides certain problems alluded to above, we may have that of the *cubic* solution of three tertiary quadratics, a solution which might become useful sometimes. That I have given a curious example of the *depression* of a certain form of equations, at pp. 45, 46 of the recently published Supplementary number of the *Mathematician*; and that, with respect to the solution of equations of the fifth degree, I have at present nothing to add to the remarks which I made in the *Philosophical Magazine* for December 1849 (S. 3. vol. xxxv. pp. 436, 437). Similar obstacles to that presented by equations of the fifth degree occur in other departments of knowledge. In the theory of elliptic functions, for instance, investigation was arrested by the impossibility of making a sum of *two* integrals fulfill certain conditions. But Abel obviated this by employing a sum of more than two integrals. I do not mean to say that the equation of the fifth degree is solvable, but I must protest against its supposed insolubility checking all inquiry in that direction. Those equations are probably destined yet to fill an important place in science.

LXX. *Some Remarks on the Theory of Thunder-storms.*
By REUBEN PHILLIPS, Esq.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I THINK it necessary to offer the following remarks on Mr. Peter Clare's theory of thunder-storms, page 334 of your last Number.

Passing over the confusion which exists in the use made of the terms "quantity or intensity," I would especially direct attention to the following grave objections:—

1. There are no experiments which show that steam can absorb electricity, and render it latent in a manner analogous to latent heat, or in any other way.

2. No source is assigned whence the electricity is to come to satisfy this capacity for electricity.

3. The theory requires the production of positive electricity without negative.

Then with regard to the theory of rain, page 335, it is forgotten that, in the interior of a cloud, each particle is similarly related to surrounding particles, and that consequently they do not move each other in the way supposed. The truth is, the whole electrical force of the cloud is thrown upon the surface of the cloud, as was long ago pointed out by Dr. Faraday*. From which it follows, that aerial condensations must occur in the lines of electrical induction; but, judging from the appearances of a thunder-storm, these condensations are so small in amount as to be incapable of producing those effects which Mr. Clare suggests, although sufficient, as has been long supposed, to produce winds and move clouds. Mr. Clare also forgets that rain *precedes* the flash of lightning.

Evaporation has sometimes been supposed to generate electricity; but it having been found by Mr. Armstrong, a result subsequently confirmed by Dr. Faraday, that by allowing the steam to escape, as on removing the safety-valve from a boiler, no electricity is obtained, it appears necessary to abandon the idea, and to agree to consider, with M. Peltier, that the electricity apparently produced by evaporation under certain circumstances must be ascribed to other causes, such as the decrepitation and projection of saline matter; and in experiments where pure water only is used, to friction between the water and other bodies, steam or particles of water carrying off one species of electricity.

The theory of atmospheric electricity which I have ven-

* Phil. Mag. vol. xxii. p. 203.

tured to give does not rest on vague surmise or on doubtful experiments, but on many clear and independent experiments, which harmonize well together. [I allude here only to the new modes of electrical excitement I have discovered, and not now to the magnetism of steam.] The experiments I have recorded appear to me undoubtedly to demonstrate, that water, rubbing against either air or steam, becomes positive. For instance, in the experiment with the fountain (107.), the water passes from the jet, dashes against the metal, and becomes reduced to a number of minute drops, and thus exposes a large surface; much of the motion of the water is then soon given up to the air through friction, and the drops are very feebly charged with positive electricity in consequence. The drops now come in contact with the sides of the tin pipe, and running together exalt the electric tension so far that the electricity becomes capable of being registered by the electrometer.

If we consider a mass of air and drops of water moving with a uniform velocity in a straight line, it is immediately seen that this motion cannot disturb the relative position of any particle. Suppose now the motion of the air to be deflected into a curve, and it is quite certain that curved currents do exist in the atmosphere, then from the great difference of specific gravity between air and water, the water would continue to separate from the air in lines more or less approaching the tangents of the curve along which the neighbouring air is moving. Now if the condensation of vapour continue to go on in the space inside the curve, the particles so condensed would be whirled to, and accumulated on the convex side of the curve, being by the friction positively electrified, and leaving the air through which they passed negative. We may suppose this process to go on, and the electric tension of the cloud consequently to rise until rain begins to fall. Now the drops falling from the superior portions of the cloud will increase in size in falling through it, and will therefore be larger on leaving the cloud than the drops formed at its inferior portions; these larger drops, in falling towards the earth, will therefore more or less overtake the smaller drops, or, in other words, when the rain falls the electrified particles become approximated.

In the storm with which Dublin was some time since visited*, the difference of motion between the wind and water or ice was evidenced by a noise. And the remarkable appearances described by Mr. Clare in the last Number could, I suppose, only have been an exhibition of the negative elec-

* Phil. Mag. vol. xxxvi. p. 396.

tricity left in the atmosphere after the positive had been discharged in the shape of lightning. Mr. Joule appears to take this view of the case*. The account of these discharges is also especially interesting, as they seem to have been in many respects a near approach to an aurora. It is a pity no experiments could be made to ascertain the state of the air.

I would say, in conclusion, I should like to see brought forward in this Journal any fair objection to my explanation of the cause of lightning and the aurora.

I am, Gentlemen,

Your very obedient Servant,

7 Prospect Place, Ball's Pond Road,
November 6, 1850.

REUBEN PHILLIPS.

LXXI. *On the Positive Wave of Translation.*

By A. J. ROBERTSON, *Civil Engineer*.†

[With a Plate.]

IN the following pages is discussed, not the theory of waves under its most general form, but merely one of the simplest cases—the Wave of Translation or Wave of the First Order. To determine from the general laws of fluids what must be the effect of the protrusion of a solid into a given body of water, and to give an equation which shall be an expression of all possible cases of wave motion, would be undoubtedly far more useful and far more satisfactory. But in the absence of the capability of attaining so high an object, it is hoped that, although far from perfect, the following investigation may not be wholly in vain. In order that the reader may be in possession of those experimental results upon which it is founded, the following passage is extracted from Mr. Scott Russell's first report on waves to the British Association‡.

“This wave has been found to differ from every other species of wave in the motion which is given to the individual particles of the fluid through which the wave is propagated. By the transit of the wave the particles of the fluid are raised from their places, transferred forwards in the direction of the motion of the wave, and permanently deposited at rest in a new place at a considerable distance from their original position. There is no retrogradation, no oscillation; the motion is

* Phil. Mag. vol. xxxvii. p. 128.

† Communicated by the Author.

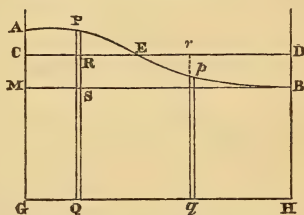
‡ Seventh Report of the British Association, 1837, p. 423. For further particulars of the wave of translation see Mr. S. Russell's Second Report on Waves contained in the Report of the British Association for 1844.

all in the same direction, and the extent of the transference is equal throughout the whole depth. Hence this wave may be descriptively designated the Great Primary Wave of Translation. The motion of translation commences when the anterior surface of the wave is vertically over a given series of particles; it increases in velocity until the crest of the wave has come to be vertically above them, and from this moment the motion of translation is retarded, and the particles are left in a condition of perfect rest at the instant when the posterior surface of the wave has terminated its transit through the vertical plane in which they lie. This phænomenon has been verified up to a depth of five feet."

Lemma I. Let the vessel AH contain a quantity of water the surface of which is any curve AEB.

Let PQ *pq* be vertical elementary columns of the water, and let CD be a horizontal line bisecting the difference of level of A and B.

Let $y=f(x)$ be the equation to the curve; then the accelerating force in a horizontal



direction on every particle in the column PQ is $g \frac{dy}{dx}$; for

$\frac{dy}{dx}$ expresses the ratio of the difference of two consecutive ordinates to the distance between them—the moving force to the mass moved. Every particle in the column PQ is likewise pressed upwards by a force represented by AC—PR. They are pressed downwards by a force PS=PR+RS, therefore downward pressure

$$= PR + RS - (AC - PR) = 2PR, \text{ for } RS = AC.$$

In the column *pq* the pressure is upwards and $= 2pr$.

Therefore generally, calling the upward pressure negative and the downward positive, the vertical pressure on any column equals the weight of a column of the same section, the length of which is twice the distance of its summit above the line CD.

Hence at E there is no vertical pressure.

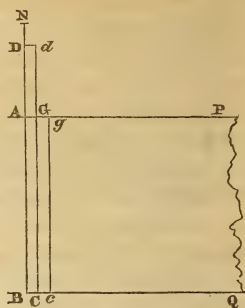
Lemma II. The oscillation of an exceedingly thin vertical column of fluid, urged by a force varying as the distance of the top of the column from a given point, is isochronous.

Let AgcB be a column of water.

Let there be a thin plate between this column and the

water in the vessel APQB, the plate being capable of horizontal motion.

Let DC be the column when raised above the level of repose, and let the water in the vessel be supposed to have no influence on the water in the column DC, except exerting a pressure upon it; because if this water were to be put in motion by the motion of the column DC, the conditions would be materially altered from those assigned in the enunciation.



The unbalanced pressure on the column DC = weight of column DG.

Let area of column	$Ac = \beta$	} $AD = x.$
And the thickness = unity.	$AB = a + k$	
Let the distance from which motion begins	$AN = k$	

In order that the surface Dd may fall, the plate must move horizontally from AB, the area of the column being constant; and the movement of the top and bottom of the plate will be the same, because the pressure on every part of it is the same. Although the surface Dd falls, the particles at BC are, as regards vertical motion, at rest; and the vertical movement of any intermediate row of particles is proportional to its distance from B.

In the same manner the surface dC moves horizontally whilst the surface DB is at rest, and the horizontal movement of any intermediate row of particles is proportional to its distance from the line DB.

Now the inertia of a column DB, the particles of which have a motion varying from a given quantity γ at the top to nothing at the bottom, is one-third of that of a similar column every particle of which is moved through γ , and l being the length, inertia = $\frac{\gamma^2 l}{3}$.

The inertia, as regards horizontal movement likewise, is $\frac{1}{3}$ of what it would be if every particle had the same motion as the surface dC; and the total inertia is the sum of the two.

Let the surface Dd fall through space Δx , and let Δy be the corresponding horizontal movement of dC, then

$$(DB - \Delta x)(BC + \Delta y) = DB \times BC,$$

$$\therefore \Delta y = BC \frac{\Delta x}{DB - \Delta x} = BC \frac{\Delta x}{DB},$$

since Δx is exceedingly small; but

$$BC = \frac{\beta}{(a+k)+x},$$

and

$$DB = a + k + x;$$

therefore $\frac{\Delta y}{\Delta x}$, and ultimately

$$\frac{dy}{dx} = \frac{\beta}{\{a+k+x\}^2},$$

which is the velocity in the horizontal direction when that in the vertical is unity.

$$\text{Inertia in vertical direction} = \frac{\beta}{3};$$

$$\text{inertia in horizontal direction} = \frac{\beta}{3} \left\{ \frac{\beta^2}{(a+k+x)^4} \right\};$$

$$\therefore \text{total inertia} = \frac{\beta}{3} \left\{ 1 + \frac{\beta^2}{(a+k+x)^4} \right\}.$$

Moving force = weight of column

$$DG = \frac{\beta}{a+k+x} \cdot x;$$

$\therefore$ accelerating force

$$\mu = \frac{\frac{\beta x}{a+k+x}}{\frac{\beta}{3} \left\{ 1 + \frac{\beta^2}{(a+k+x)^4} \right\}} \cdot g = 3g \cdot \frac{\frac{x}{a+k+x}}{1 + \frac{\beta^2}{(a+k+x)^4}}.$$

The column being exceedingly thin, β is exceedingly small, and *a fortiori* $\frac{\beta^2}{(a+k+x)^4}$. This quantity may therefore be neglected; and when x is small in comparison with $a+k$,

$$\mu = 3g \frac{x}{a+k} = -\frac{d^2x}{dt^2},$$

as the effect of μ is to diminish x .

Integrating, remembering that when $x=k$ the velocity is nothing, and that x diminishes as t increases,

$$t = \sqrt{\frac{a+k}{3g}} \cos^{-1} \frac{x}{k} \quad . \quad . \quad . \quad . \quad (3.)$$

2 L 2

When $x=0$,

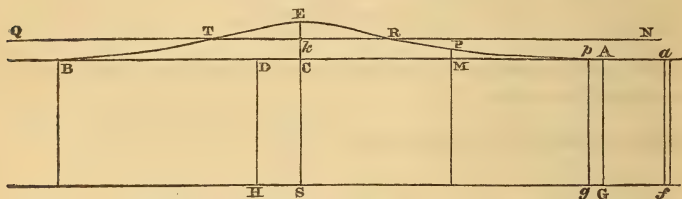
$$t = \sqrt{\frac{a+k}{3g}} \cdot \frac{\pi}{2} \quad \dots \quad (4.)$$

When $x=k$ or $-k$, $t=0$,

$$\text{time from rest to rest} = \sqrt{\frac{a+k}{3g}} \cdot \pi;$$

and this being independent of k , considering $AB=a+k$ as constant, the oscillation is isochronous.

The Wave of Translation.



Let AEB be a wave, AB the level of repose, QN a horizontal line bisecting EC in k .

$$\text{Let } AM=x \quad \left. \begin{array}{l} AC=L \\ PM=y \end{array} \right\} EC=2k \therefore Ek=kC=k$$

$$AG=a$$

$$CD \times DH = \frac{1}{2} \text{ volume of generator} = V$$

$$\therefore CD = \frac{V}{a}.$$

Let af be a thin column taken anywhere in the still water. The pressure on any particle is as its depth below the surface, but being the same in all directions, the particle remains at rest. But at A, the exact commencement of the wave, we have on one side of AG a column pg slightly higher, whilst that on the other side has the same height; there is therefore an unbalanced pressure in direction Aa . The immediate effect on the column AG is to raise A; for until the pressure can be communicated through it to the water beyond, that water is the same as if it were solid. The column AG when raised exerts a pressure in its turn upon the next one, and causes it to rise in the same manner. Thus the commencement of the wave travels forwards with a velocity which depends on the ease with which the pressure can be propagated. This ease depends partly on the resistance to being raised offered by the column itself, and partly on the forward move-

ment which must take place in all the water behind, in order that the pressure upon the column may be sustained. When the column AG rises, it necessarily becomes thinner; whilst *pg*, moving forwards to supply the place which would otherwise be left vacant, exerts a pressure on AG, it is itself pressed by a higher column; and as it can move forward only so far as the horizontal diminution of AG permits, it also rises—and so with every column behind. The further it is from A, the greater evidently must be its horizontal motion for a given upward movement of column AG, because it must pass through a space equal to the sum of all the compressions in front of it, and this is true of any column between A and the crest E of the wave.

We have seen before, in the case of the water contained in the vessel, that the accelerating force forwards at any point is proportional to $\frac{dy}{dx}$ —the tangent of the inclination of the sur-

face at the point; also that there is an upward pressure, which varies as the distance of the column below the line QN. At the commencement A of the wave, the conditions have been shown to be the same for the instant as if the water were in a vessel of which the end is solid—the pressure is wholly upwards. As we recede from A, the motion forwards requires to be greater and greater; and the slope of the surface increases as well, until about the point R it attains its maximum value. On the other hand, the upward pressure becomes less and less, until at the point R there is no vertical pressure at all. The forward motion then arises only from the compression of the columns, and is limited by it; but the rising of the columns in succession evidently produces that change in the position of the wave-form which gives the water the appearance of actual forward motion; and if the wave be divided into an exceedingly great number of vertical columns, each of these columns represents a stage that every one of the others either has passed through or has yet to pass through. Since either of the motions of a body urged by one force in a vertical, and by another in a horizontal direction, is independent of the other, the vertical motion of these columns will be the same, and may be examined as if they had no horizontal movement.

It appears, then, that the column AG is urged upwards by a force proportional to *k*, and diminishing as the distance of its summit from QN diminishes, until at R it is nothing. It in consequence acquires a certain velocity, by virtue of which it continues to rise and get thinner. Now, however, there is a retarding force acting downwards, which, as it varies in the same proportion as the accelerating force does, destroys the

motion in equal space and time. When the surface rises to E it comes to rest. The downward force continuing to act, becomes an accelerating force; at T the velocity is a maximum: again it is retarded; and when the surface has sunk to B, it is again at rest. It is to be borne in mind, that whilst the uppermost particles in this column have been moving through a vertical distance A to E, the lowest particles have not been moving at all; and the intermediate particles have had an intermediate movement, as in the case of the single column. Also taking the columns sufficiently small, the horizontal motion of the particles in each may be disregarded, their total effect being the horizontal movement of the whole column.

From equation (4.) we have seen that the time of rising from A to E by a single column, urged by a force varying as above, is proportional to $\sqrt{\frac{a+k}{3g}}$; $\frac{3g}{a+k}$ is the absolute force on the column at a distance unity from QN. But the force which urges the column of the wave cannot be the same as this, as it is modified by the horizontal motion of the rest of the columns. It must nevertheless vary in the same proportion. Let the force be μ , then

$$\mu = c \cdot \frac{g}{a+k},$$

the value of c remains to be determined.

Every column between the commencement and crest of the wave is becoming continually narrower and higher; therefore, although the inclination of the surface is greatest at R, the forward motion is greatest at E. But at R the velocity of the column upwards is at its maximum; the *rate*, therefore, at which it shrinks is greatest. After the crest of the wave has passed, the horizontal force becomes a retarding force, and destroying the motion in the same time as it had communicated it, the end of the wave finds the column at rest*. What, then, is the quantity of this motion?

* The peculiarity of the wave of translation appears to be this—the horizontal and vertical movements commence and end together. Suppose that by some means a backward horizontal motion had been communicated to the column AG, such that by the time the surface has risen to R it is destroyed. Whilst the path of the particles in space is altered by this supposition, the relation between the vertical and horizontal movements remains the same. The shrinking of the columns would only be towards R in place of towards A: at R the upward movement would still be a maximum. The horizontal motion would then be reversed, acquire a maximum at E, be destroyed at T, and reversed again. The motion being therefore alternately backwards and forwards, the particles oscillate about a fixed

Since the water in the channel remains at the same level as before the passage of the wave, the volume of the generator is contained in the swell of the water above the level of repose; and calling the width of the channel unity, the number of units in the area of the curve ARETB above AB equals the number of units in the volume of the generator. The column DH, at rest at the commencement of the wave, has been moved forward by the time that D has risen to E, or when the crest of the wave has come over the column DH, a certain distance DC, which is the sum of the contractions of all the columns in front; and since the water formerly contained in DAGH is now contained in ERAGS, $DC = \frac{V}{a}$.

The total horizontal motion is therefore $\frac{2V}{a}$.

Both vertical and horizontal motions now described as the result of existing forces, are precisely those observed by Mr. Scott Russell in his experiments on the wave of translation.

From the nature of the case, the motion of the wave must be uniform. If then T be the half-period of the wave, and t an intermediate time, the distance passed through in t is

$$\lambda = \frac{t}{T} \left(L + \frac{V}{a} \right),$$

point; and, moreover, as the horizontal and vertical motions cross one another, and the particles do not come to rest after the passage of one wave, there must be a succession of waves. The following figures show the difference between the motions on this supposition.

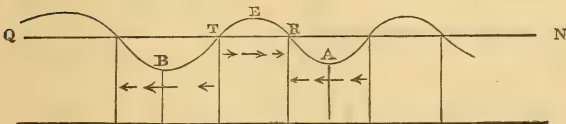
Wave of Translation.



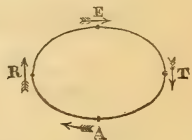
Path of particle.



Wave of Oscillation.



Path of particle.



The law of the diminution of horizontal motion, according to the depth of the particle below the surface in an oscillating wave, the author is unable to determine.

which is also the length of the body of water which has been compressed in time t , the whole length in time T being

$$AD = L + \frac{V}{a}.$$

Also the force causing each column to rise varies as the distance from the line QN ;

$$\therefore t = \frac{\cos^{-1} \frac{k-y}{k}}{\sqrt{\mu}} = \frac{\text{versin}^{-1} \frac{y}{k}}{\sqrt{\mu}},$$

and

$$y = k \text{ versin } \sqrt{\mu} \cdot t.$$

Let $\delta\lambda$ represent the thickness ad of an elementary portion of the water. After time δt it becomes compressed into a smaller width $\delta\lambda_1 = ec$, or

$$ab \times ad = ec \times (ab + ac),$$

i. e.

$$\delta\lambda \times a = \delta\lambda_1 (a + k \text{ versin } \sqrt{\mu} \cdot t);$$

$$\therefore \frac{\delta\lambda_1}{\delta\lambda} = \frac{a}{a + k \text{ versin } \sqrt{\mu} \cdot t} = \frac{d\lambda_1}{d\lambda} \text{ ultimately};$$

$$\therefore \frac{d\lambda_1}{dt} = \frac{d\lambda_1}{d\lambda} \cdot \frac{d\lambda}{dt} = \frac{1}{T} \left(L + \frac{V}{a} \right) \cdot \frac{a}{a + k \text{ versin } \sqrt{\mu} \cdot t}.$$

Integrating, λ_1 being 0 when $t=0$,

$$\lambda_1 = x = \frac{L + \frac{V}{a}}{T} \cdot \frac{2}{\sqrt{\mu}} \cdot \sqrt{\frac{a}{a+2k}} \tan^{-1} \left\{ \sqrt{\frac{a+2k}{a}} \tan \frac{\sqrt{\mu} \cdot t}{2} \right\} \quad (6.)$$

When

$$t = T, \quad y = 2k; \quad \therefore T = \frac{\pi}{\sqrt{\mu}}.$$

Also

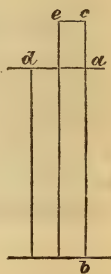
$$\sqrt{\mu} \cdot t = \text{versin}^{-1} \frac{y}{k}; \quad \therefore \tan \frac{\sqrt{\mu} \cdot t}{2} = \sqrt{\frac{y}{2k-y}}.$$

Therefore, substituting in equation (6.),

$$x = 2 \frac{L + \frac{V}{a}}{\pi} \sqrt{\frac{a}{a+2k}} \tan^{-1} \sqrt{\frac{a+2k}{a} \cdot \frac{y}{2k-y}}. \quad (7.)$$

When $y = 2k$,

$$\tan^{-1} \sqrt{\frac{a+2k}{a} \cdot \frac{y}{2k-y}} = \tan^{-1} \infty = \frac{\pi}{2},$$



and

$$\frac{L}{L + \frac{V}{a}} = \pm \sqrt{\frac{a}{a + 2k}}; \dots \dots \dots (8.)$$

from which results the following value for L :

$$L = \frac{V}{2k} \left\{ 1 \pm \sqrt{\frac{a + 2k}{a}} \right\} \dots \dots \dots (9.)$$

From (7.) and (8.) we also obtain

$$y = 2k \cdot \frac{\tan^2\left(\frac{\pi}{2} \cdot \frac{x}{L}\right)}{\frac{a + 2k}{a} + \tan^2\left(\frac{\pi}{2} \cdot \frac{x}{L}\right)}, \dots \dots \dots (10.)$$

the equation to the curve in terms of x .

$$\frac{dy}{dx} = \frac{dy}{dt} \cdot \frac{dt}{dx} = \frac{\pi k}{L + \frac{V}{a}} \cdot \sin \sqrt{\mu} \cdot t \left\{ 1 + \frac{k}{a} \text{versin} \sqrt{\mu} \cdot t \right\}. \quad (11.)$$

When $t=0$ or $=T$, $\frac{dy}{dx}=0$, or the tangent to the curve at the ends and at the crest of the wave is horizontal; therefore the curve at the ends is convex, and at the crest concave, to the line of repose. Now $g \frac{dy}{dx}$ would be the accelerating force on the supposition of the whole of that force being expended in producing forward motion. But the cause of both vertical and horizontal motion is the slope of the surface, the amount of which is expressed by $\frac{dy}{dx}$; and it follows that the definite

integral of $g \cdot \frac{dy}{dx}$ between the limits $x=0$ and $x=L$ represents the sum of all the forces producing forward motion, and of all the forces producing vertical motion. Hence we obtain an equation from which the value of the unknown quantity μ may be deduced.

The space s which a column has moved through in a horizontal direction in time t , is

$$\left(L + \frac{V}{a}\right) \frac{t}{T} - x$$

$$= \frac{L + \frac{V}{a}}{T} \left\{ t - \frac{2}{\sqrt{\mu}} \sqrt{\frac{a}{a + 2k}} \tan^{-1} \left\{ \sqrt{\frac{a + 2k}{a}} \tan \frac{\sqrt{\mu} \cdot t}{2} \right\} \right\},$$

and

$$\frac{d^2s}{dt^2} = \mu \frac{L + \frac{V}{a}}{\pi} \cdot \frac{a}{(a+y)^2} k \sin \sqrt{\mu} \cdot t = \text{force which has}$$

produced the motion.

Again, force producing vertical motion is

$$(k-y)\mu = k\mu \cos \sqrt{\mu} \cdot t;$$

$$\therefore \int_{x=L}^{x=0} g \cdot \frac{dy}{dx} = \int_{t=T}^{t=0} \frac{d^2s}{dt^2} + \int_{t=T}^{t=0} k\mu \cos \sqrt{\mu} \cdot t. \quad (12.)$$

Therefore, remembering that each side of the equation = 0 when $t=0$,

$$g \frac{\pi k^2}{\left(L + \frac{V}{a}\right) a \sqrt{\mu}} \left\{ \frac{a+k}{a} \text{versin } \sqrt{\mu} \cdot t + \frac{1}{4} (\cos 2 \sqrt{\mu} \cdot t - 1) \right\}$$

$$= \frac{\left(L + \frac{V}{a}\right)}{\pi} \cdot \sqrt{\mu} \cdot \frac{k(1 - \cos \sqrt{\mu} \cdot t)}{a+k - k \cos \sqrt{\mu} \cdot t} + k \sqrt{\mu} \sin \sqrt{\mu} \cdot t.$$

When $\sqrt{\mu} \cdot t = \pi$, or $t = T$,

$$2g \cdot \frac{\pi k}{\left(L + \frac{V}{a}\right) a \sqrt{\mu}} \{a+k\} = \frac{L + \frac{V}{a}}{\pi} \sqrt{\mu} \frac{2k}{a+2k}.$$

Whence, substituting for $L + \frac{V}{a}$ its value obtained from (8.),

$$\mu = \frac{\pi^2}{L^2} \cdot (a+k)g, \quad \dots \dots (13.)$$

and

$$T = \frac{\pi}{\sqrt{\mu}} = \frac{L}{a+k} \sqrt{\frac{a+k}{g}}.$$

Now $\sqrt{\frac{a+k}{g}}$ is the time which a body falling freely by gravity takes to fall through the space $\frac{a+k}{2}$.

Therefore the period of the half-wave is to the time of falling through $\frac{a+k}{2}$ as L is to $a+k$.

Since in uniform motion, if the times be proportional to the distances the velocities are equal, the velocity of the crest of the wave would be that acquired by a body falling

through $\frac{a+k}{2}$, if the distance traversed by it in the half-period were L . But this distance is $L + \frac{V}{a}$; hence the velocity is increased in the proportion of $L + \frac{V}{a} : L$;

$$\therefore v = \frac{L + \frac{V}{a}}{L} \sqrt{(a+k)g} = \sqrt{\frac{a+k}{a} \cdot (a+2k)g} \quad (14.)$$

It was determined before, from the oscillation of a single column, that

$$\mu = \frac{c}{a+k} \cdot g = \frac{\pi^2}{L^2} \cdot (a+k)g$$

from (13.);

$$\therefore c = \frac{\pi^2}{L^2} \cdot (a+k)^2 \quad L = \frac{\pi}{\sqrt{c}} (a+k)$$

$$= \pi(a+k), \text{ if } c=1 \text{ and } \mu = \frac{g}{a+k}.$$

There is no proof that $c=1$; but it is remarkable, that, if it be true, then the half length of the wave $=\pi(a+k)$, which agrees very exactly with the observations of Mr. Scott Russell, who has assigned from them a value $L=\pi a$. The velocity, however, is independent of the length, and consequently remains very nearly the same, although the wave becomes lower and longer†. Is not the degradation of the wave the result of imperfect fluidity?

The cause of the increase of velocity consequent upon an increase of depth is evident; the volume of the generator $2V$ being constant, the amount of horizontal movement of the particles is inversely as the depth.

The first five columns in the following table are copied from Mr. Scott Russell's Second Report on Waves‡.

The column F gives the velocities calculated by the above formula, and G gives the difference between observation and theory.

* The empirical formula given by Mr. Scott Russell is $v = \sqrt{(a+2k)g}$.

† It is remarkable, that, if these principles be applied to oscillating waves, as in the note, pp. 518, 519, where the middle line QN is the level of repose, and where the particles having no positive motion of translation, the distance travelled by the crest of the wave in the time T is only L , the velocity is that acquired in falling through half the simple depth of the water; or since the length varies as the depth, the velocity is as the square root of the length.

‡ Report of the British Association for 1844, page 336.

Column A is the mean height of the wave crest $= a + 2k$.

Column B, selected examples from which A is taken.

Column C, depth of fluid in repose in inches $= a$.

Column D, the height of waves in inches $= 2k$.

Column E, the velocity of wave observed

Column F, by formula $\sqrt{\frac{a+k}{a} \cdot (a+2k)g}$, in feet per second.

Column G, difference between E and F

Small Waves.

A.	B.	C.	D.	E.	F.	G.	No.
1·075	1·05 & 1·10	1·000	·075	1·670	1·73	·06	1
1·3	1·3	1·150	·150	1·810	1·927	·117	2
3·17	3·09 & 3·23	2·963	·207	2·860	2·965	·105	3
3·36	3·32 & 3·40	3·080	·280	2·960	3·068	·108	4
4·16	4·0 & 4·31	3·903	·256	3·310	3·390	·080	5
5·34	5·20 & 5·5	5·088	·252	3·758	3·829	·071	6
6·52	6·4 & 6·65	6·220	·304	4·094	4·232	·138	7
7·51	7·42 & 7·7	7·040	·474	4·406	4·563	·157	8
Average							·1045

Large Waves.

A.	B.	C.	D.	E.	F.	G.	No.
1·20	1·20	1·00	·200	1·760	1·880	·120	1
1·62	1·62	1·300	·320	2·060	2·209	·149	2
2·19	2·19	1·900	·290	2·300	2·514	·214	3
3·38	3·35—3·41	2·960	·420	3·010	3·116	·106	4
3·55	3·5—3·61	3·020	·532	3·080	3·219	·139	5
3·83	3·69—3·97	3·007	·830	3·252	3·419	·167	6
4·53	4·4—4·75	3·910	·625	3·505	3·622	·117	7
5·21	5·21	3·870	1·340	3·820	4·030	·211	8
5·76	5·61—5·82	5·070	·692	3·970	4·063	·093	9
6·24	6·15—6·40	5·080	1·160	4·170	4·318	·148	10
6·69	6·69—7·20	6·034	·823	4·262	4·378	·116	11
Average.....							·143

Or, leaving out the two errors ·214 and ·211, the average would be ·128. In the eighth example, the error might have been attributed to the circumstance of the wave being higher; but the error in the third example is as great, although the height of the wave is only ·29. The twelfth example, given by Mr. Scott Russell, is evidently anomalous.

From this comparison it appears that the average difference between the velocity observed and that assigned by the formula is scarcely more, in the case of the small waves, than

1 inch per second on velocities varying from 1 foot 9 inches to 4 feet 6 inches per second; and in the case of the large waves, the error is $1\frac{3}{4}$ inch per second. This discrepancy, moreover, is on the right side; as viscosity and friction have undoubtedly some influence, although slight, and will make the actual velocity less than a theory which does not recognize them can assign.

In order to determine whether anything was due to viscosity, the author tried the following experiment:—In a trough 2 feet 2 inches long by $1\frac{1}{2}$ inch wide, a wave was generated, by pushing down at one end a block of wood, 2 inches long and of a width equal to that of the trough, first in water, then in linseed oil. The distance, then, for the wave to travel was 2 feet exactly. The quantities of water and oil were measured in the same vessel, and therefore the depths were the same, and all circumstances alike except the nature of the fluid. In the water the undulation could be distinctly perceived after the wave had travelled 128 feet, but in the oil it was only just perceptible when it had travelled but 12 feet. The time occupied by the wave in the water in passing over the above distance was 1 minute; by the wave in the oil in passing over 12 feet, fully 6 seconds. Taking it at 6 seconds, $\frac{12}{6} = 2$ feet per second. Water wave $\frac{128}{60} = 2.13$ feet per second.

The height was, as nearly as could be ascertained, 1.65 inch, which would give a velocity of 2.104 feet per second: much dependence, however, could not be placed on this measurement.

The accompanying diagram (Plate III.) is half of the wave, No. 7 of the large waves, laid down full size from calculation by the formula (10.), assuming the length to be $\pi(a+k)$.

LXXII. *On the Explosiveness of Nitre, with a view to elucidate its agency in the tremendous explosion of July 1845, in New York.* By ROBERT HARE, M.D., Emeritus Professor of Chemistry in the University of Pennsylvania, and Associate of the Smithsonian Institution*.

1. **A**MONG the conflagrations by which cities have been more or less devastated, there has been none, it is believed, of which the phænomena were more awful and

* This memoir forms one of the Smithsonian Contributions to Knowledge, and was accepted for publication by the Institution, October 1849. For its communication we are indebted to the kindness of the Author.

mysterious than those of the great fire which took place in the city of New York, on the 19th of July, 1845.

2. The destruction of 230 houses, containing merchandise amounting in value probably to 2,000,000 of dollars, made the calamity in question highly deplorable as a cause of pecuniary loss and embarrassment; while the characteristics which gave to it an unprecedented rapidity of extension were of a nature to excite an enduring interest as well as temporary consternation.

3. A series of detonations, successively increasing in loudness, were followed by a final explosion, of which, agreeably to an affidavit, the report resembled a "*loud clap of thunder*." This tore into pieces the building within which it took place, threw down seven houses in the vicinity, and drove in the fronts of the houses on the opposite side of the street, at the distance of 87 feet. The whole of the space within which these tremendous effects took place, was filled with a dazzling flame, and various masses, intensely ignited and vividly luminous, were projected aloft as if expelled from a volcano, so as, on alighting, to spread the conflagration far and wide. Shipping anchored in the Hudson River, probably at the distance of more than a quarter of a mile, were greatly endangered by these deflagrating missiles.

4. So violent was the atmospheric concussion, that people were prostrated by the consequent blast, when too remote to be injured by the flames or flying fragments. Some persons were wounded or killed, but so small was the number, in comparison with that of the multitude which might have been mutilated or destroyed, that there was more gratulation for the escape of the many, than sorrow over the few who actually perished. This comparative immunity was due to the warning given by the detonations, which, as already mentioned, preceded that by which the mischief was effected.

5. The natural inference arising from the detonations thus alluded to, was, that gunpowder had been stored in parcels of various amounts on the different floors of the store, the smaller portions above, the larger below, and that the detonations were the consequence of the successive ignition of the parcels thus situated. The cry of gunpowder was raised on the occurrence of the first explosion, and caused the retreat of almost everybody near the quarter whence it proceeded. Hence, before the final catastrophe, the streets about the store were entirely vacated, so that scarcely any person was injured besides those in the houses opposite to the conflagration.

6. Notwithstanding the reasonableness of the belief at first created as to the agency of gunpowder, there was the most

conclusive evidence, so far as the oaths of worthy and well-informed witnesses could avail, that no gunpowder was contained in the building within which the explosions occurred. Of course, the real cause of the disaster became a subject of perplexing consideration for chemists in general, and especially for those adepts in chemistry, to whom the Corporation of the city concerned applied for an elucidation of the mystery.

7. It was fully established by the statements of the highly respectable proprietors, and that of their storehouse clerk, that there were in the store more than 300,000 pounds of nitre, secured in double gunny bags, containing 180 pounds of nitre each, in piles alternating with heaps of combustible merchandise; yet as, agreeably to ordinary experience, such combustibles deflagrate when ignited with nitre, without exploding, this did not remove the unfavourable impressions unjustly created respecting the occupants of the store. The stowing of any large quantity of gunpowder adequate to the effects produced, had been culpably imprudent and illegal; and coupled with a most solemn denial on their part, would have involved them in the baseness of falsehood, if not the guilt of perjury.

8. To everybody the elucidation of the mystery was desirable, since, without a correct knowledge of the causes, the proper means of guarding against a recurrence of such explosions could not be devised. It was interesting to men of science to have it ascertained wherefore their efforts to produce an explosion by similar ingredients were unsuccessful*. To the occupants of the store it was important, since they were liable, not only to ill opinion and legal prosecution as above stated, but likewise to a deprivation of their claims for insurance. Fortunately for them, in opposition to the opinions and experimental inferences of several chemists who were consulted, tending to extend or confirm the idea, that gunpowder, illegally and most culpably stored, must have been the cause of the catastrophe, the opinions of Silliman and Hayes, and other eminent chemists, were called forth, tending to sanction the inference that the result might be due to the reaction of nitre with contiguous merchandise.

* With means furnished by the Councils, five eminent chemists made several experiments upon a large scale, in order to ascertain the effect of igniting nitre with such combustibles as were associated with it in the store of Messrs. Crocker and Warren; yet in no instance could they produce detonating reaction. The activity of the combustion never surpassed that degree of rapidity and consequent violence which may be designated by the word deflagration.

9. I owe it to my friend, Augustus A. Hayes, to state, that I might have adopted the more general impression, had it not been for his inferences and experiments made with the view of accounting for the explosion of a vessel, loaded with nitre, while lying at anchor in the harbour of Boston. It was ascertained by this able chemist, that when, by an experiment made in his laboratory, between 100 and 200 pounds of nitre, intensely heated in a crucible, were suddenly sprinkled with water, an explosion ensued*.

* The subjoined quotation from the Boston Daily Advertiser, will serve to show the point to which Mr. Hayes had attained in elucidating the mysterious explosions produced by incandescent nitre, when my efforts to afford a further elucidation commenced.

Roxbury Laboratory, 28th July, 1845.

Dear Sir,—Your note of yesterday, in relation to the explosive action of saltpetre, has this moment come to hand. I most cheerfully comply with your request in placing before you the facts connected with the subject of the action of saltpetre on substances usually called combustible.

Saltpetre, or the nitrate of potash or soda, *alone*, does not burn or explode by heat, however intense. It parts with one of its constituents, oxygen, by heat, and it is to the combination of its oxygen with other bodies that it owes its power of burning with them. Wood and other fibrous substances do not burn with saltpetre until they have become partially charred; they then produce *deflagration*, or burn with sparks. A large quantity of saltpetre, enclosed in gunny bags, as it is usually stored, after fire was communicated to it, would burn with the bags, emitting much smoke and sparks, precisely as paper which has imbibed saltpetre would. It would not be consumed; only the small quantity required to burn with the bags would be changed. If an addition of burning wood or charcoal were made to the extent of one-fifth the weight of the saltpetre, an intense and continued deflagration would result, and all the saltpetre would be changed. *No explosion would follow from applying fire to mixtures of charcoal, or wood and saltpetre*; the rapid combustion called deflagration would be produced, but, unlike explosion, time would be required for the mutual actions; and where the quantities were large, many hours would be necessary before they would cease. The recent destruction of life and property in New York, the loss of a homeward-bound Indiaman and her cargo, by a similar cause, have created an anxiety which has led to many inquiries respecting the origin of the *explosions* attending the burning of saltpetre. I need not remind you of a case which occurred at Central Wharf, about ten years since, when the Hartford Packet was destroyed. The testimony obtained in the last instance led me to make some experiments on the effects produced by dropping water on a burning mixture of saltpetre and charcoal. It was ascertained that a very small weight of water, relatively to the saltpetre, caused explosions, which might be made successive, so long as the materials remained. The quantities of the substances acting being increased to between 100 and 200 pounds, the addition of water, in the form of spray, caused an explosion which destroyed the vessel, and shook all the buildings in the vicinity. The temperature of a burning mixture of saltpetre and charcoal, at the points of contact, is superior to that of "white hot" iron, and the form is that of a bubbling fluid. Water falling on the

This statement of Hayes caused me to recollect, that upon one occasion a mischievous explosion had occurred in my laboratory, when a fissure taking place in an iron alembic holding about twenty pounds of fused nitre, on hoisting the alembic off the fire, a jet of the liquefied salt fell accidentally upon some water in a tub, which was unfortunately too near. It also brought to mind that potassium, when thrown upon the surface of water, is, by combustion with the oxygen of that liquid, converted into a fused globule of red-hot oxide, which, in the act of combining with water, detonates violently. This detonation struck me as being clearly owing to a sort of double reaction, in which, while one portion of water, by uniting with the oxide of potassium, converts it into hydrate of potash, another portion, uniting with the heat, flies off explosively as steam.

10. In a letter to Hayes, immediately after his explanation appeared, I stated these facts and inferences; and moreover, I endeavoured to illustrate the subject by referring to the explosion so frequently produced by blacksmiths, through the forcible contact with moisture, of incandescent iron struck by a hammer. It has been ascertained that globules of oxide of iron, as they fall in a state of fusion from a wire ignited in oxygen, do not at first produce any commotion in water. This arises from the generation of a protecting atmosphere of rarefied aqueous vapour, which renders contact with the liquid water impossible. Widely different would be the result, were the liquid suddenly forced into contact with the globule by a blow from a hammer, as above mentioned. Analogous causes operate when globules of the most volatile liquids or solids are retained for a time in the cavity of an incandescent metallic

mass is instantly converted into steam, having the elastic force of that used in steam-guns; exceeding gunpowder in destructive energy. The red-hot particles, dispersed by the sudden action, pass over considerable spaces, and the appearance of flame is produced.

In cases where water falls on highly heated polished surfaces, such as melted glass, copper, or silver, steam is formed rapidly, but silently; the water does not touch the hot surface. The spreading of a film or crust over the polished surface instantly alters its relation to water, and causes steam to form with explosive violence, attended by a loud report.

I do not hesitate in expressing my belief, that the disastrous effects produced in New York were caused by water or other fluid falling on saltpetre, while burning with the bags investing it. The facts which I have stated may have interest or importance in connection with attempts made to extinguish fire in buildings containing saltpetre. The danger of throwing water on the fire is manifest, while the loss to the owner of the saltpetre would doubtless be greater from water than from fire.

Respectfully,

A. A. HAYES.

Henry Williams, Esq.

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ladle, meanwhile evaporating much more slowly than if the temperature were less. In any one of these instances an explosion would follow from a contact being coerced between the heated surface and the liquid. When a hammer is employed as above described, mechanical force produces that contact, which, in the explosive union of incandescent oxide of potassium with water, is caused by intense chemical affinity.

11. The explosion produced by Hayes, as above mentioned, and that which took place in my laboratory, as well as the explosive reaction of oxide of potassium with water, gave a practical confirmation to the inference, that the meeting of water with the base of incandescent nitre could cause tremendous results. Subsequently, in the winter of 1845-46, I found that when nitre, by the flame of a hydro-oxygen blow-pipe supplied with atmospheric air and oxygen, is heated to incandescence, and then quickly submerged in water previously situated beneath the containing ladle, a sharp explosion ensues. I found, nevertheless, that when thrown, under like circumstances, upon molasses or sugar, the effects were those of deflagration rather than explosion. Yet, latterly, I have fallen upon contrivances, by which pulverized sugar and nitre may be made to explode. The first expedient which succeeded, was that of pouring melted sugar upon the face of a hammer, so as to make a disc of commensurate size. Such a disc, if it should not adhere, is easily made to do so by slightly moistening the face of the hammer. Some nitre was put into a thin shallow platina capsule, situated over a small anvil, near one of its edges, so that the bottom of the capsule might be reached obliquely by a hydro-atmospheric blowpipe flame. Under these circumstances, the nitre having been heated until its potash began to be volatilized, was struck with the sugar-faced hammer. A smart detonation was the consequence. This experiment may fail sometimes from the blow not being properly given; from the nitre not being sufficiently hot; or the capsule being ill-situated. The explosion of fulminating mercury by a hammer fails sometimes, from the blow not being so given as to produce a due degree of parallelism between the surfaces.

12. Another method of producing explosive reaction is as follows:—Nitre and sugar being coarsely powdered, let discs of paper about 3 inches in width be prepared. Place one of the discs upon an anvil, and cover it with a stratum of sugar. Then cover the sugar with a stratum of nitre, placing over this another of the discs. Heat a flat iron bar, wider than the discs, to a welding heat, and quickly withdrawing it from the fire, and holding it above the paper, strike it down thereon

with a sledge. An explosion will ensue, with a very loud report. Of course the operator's face should be protected by a mask, his hands and legs by a leathern or woollen apron and gloves. The operation may be performed by one person, but more advantageously by two, as it is difficult for one to hold the iron in the position most suitable for bringing the surfaces together with the requisite degree of parallelism.

13. In a letter in reply to one from Mr. Durant, of New York, respecting the explosions which are the principal objects of these communications, I adverted to the superiority of the affinity which exists between water and oxide of potassium over that which exists between nitric acid and the same base*, as a reason why the presence of the elements of water,

* It may be well here to advert to the fact, that one of our young countrymen, Tilghman, has, without any hint from me, not only perceived the property of water on which I have insisted, but likewise has had the sagacity to suggest its application to various useful processes. These have all been founded on that superior affinity of water for certain bases, on which, in my letter to Hayes, I had insisted as affording the rationale of the explosion of nitre either with this liquid, or with any substance containing its elements.

From the preceding suggestions, and some experiments, of which an account will be subjoined, it appears that the explosive violence of a mixture of nitre with substances containing carbon in union with hydrogen, or with hydrogen and oxygen, so as to be competent to convert the base into a hydrate or carbonate, is dependent on the force with which they may be held or brought together in a state of ignition, being sufficient to permit of that increase of temperature which is necessary to explosive reaction.

Probably at the temperature thus alluded to, the ingredients are all in a condition analogous to that of a very dense explosive gaseous mixture. It is well known that such mixtures detonate with a velocity apparently not less than that of an electrical discharge. A single electrical spark, a particle of platina sponge, even a sunbeam, may cause an explosion so instantaneous, that it is the collapse only that can be observed. The dilatation which precedes the collapse escapes scrutiny. However large the volume, ignition in any one part seems to affect the whole at once.

I infer, then, that when nitre and certain compounds of carbon with hydrogen and oxygen reach a temperature at which the whole mixture, if not restrained mechanically, would take the aëriform state by a sudden revolution in the electro-chemical polarities, that detonating combination ensues, to which, when ignited, various gaseous mixtures are liable. A few cubic inches of olefiant gas, with twice the bulk of oxygen, included in soap-bubbles and inflamed, will produce a report equal to that of a musket. The accidental explosion of a half-gallon of a similar mixture created a thundering noise like a field-piece, so as to alarm the whole neighbourhood within a furlong of my laboratory.

Aware of the influence of confinement in augmenting the force of reaction between nitre and combustibles, the distinguished chemists above mentioned (as having been called upon by the Corporation of New York to investigate the phenomena under consideration) treated the absence of this condition as a reason for discrediting the idea that the reaction of nitre with combustibles could account for them. But agreeably to the

or of hydrogen in union with carbon, should, on ignition with nitre, give rise to explosive reaction. Obviously, the consequence of the displacement of nitric acid by water must be, that the gaseous constituents of the acid, incapable of remaining in combination without a base, would escape, either as nitrogen, nitric oxide, or oxygen gas, or, carbon being present, partially, as carbonic oxide, or carbonic acid.

14. Gum, sugar, starch, and lignin, consist of carbon in union with the elements of water, being virtually hydrates of carbon. Oils, resins, or bitumens, consist of carbon and hydrogen, with but little oxygen. Of course either, when heated with nitre, can supply water to the base of this salt, *with*, if not *without*, assistance from its oxygen, which constitutes nearly half of the matter in nitre. Such substances may, therefore, under favourable circumstances, perform the part performed by sulphur in gunpowder, which I conceive to be that of seizing the potassium and liberating the acid, so as to enable its oxygen to react freely with the carbon and the resulting sulphide of potassium. Considerations analogous to those advanced respecting the agency of the elements of water in exploding with nitrates, will apply with respect to those of carbonic acid; since carbonated alkalies, no less than the hydrated, being indecomposable *per se* by heat, carbon as well as hydrogen must, by uniting with one portion of the oxygen of the nitric acid and taking hold of the base, expel all the nitrogen with the rest of the oxygen.

15. Having submitted the preceding facts and considerations, my explanation of the stupendous explosion which forms the topic of this communication is as follows:—

Of the enormous quantity of nitre which the store held, more than 56,000 pounds were on the first floor, about 180,000 pounds on the second floor, and about 100,000 on the third floor. The weight of combustible merchandise was about 700,000 pounds. As it was alleged by some of the witnesses examined that the iron window shutters of an upper story became red-hot by the conflagration of an adjoining house, it is probable that fire was communicated to some of the gunny bags holding the nitre, or some other combustibles, which, as stated in evidence, were piled against the shutters. As soon, however, as a single bag became ignited, the nitre

facts, I propose to show that there must have been a mechanical force in operation sufficient to bring the matter into a state analogous to that which enables fulminating combinations, or explosive mixtures of gas, to detonate either from ignition, from exposure to an electric spark, or, in some instances, from a blow or catalysis; in other words, from some influence like that exercised by platina sponge.

with which the inner bag must have been imbued, would give the greatest deflagrating intensity to the consequent combustion; while the interstices between the bags, like those between grains of gunpowder, would enable the flame to pervade the whole heap of bags. As nitre fuses at a low red heat, very soon a great quantity, in a state of liquefaction, must have run down upon the wooden floor, which would immediately burst into an intense state of reaction with the oxygen of the salt. To this combustion the merchandise adjoining would add fuel, causing a still more extensive liquefaction of the nitre. The deflagrating mass thus created, on burning its way through the floor, or falling through the scuttles, which were all open agreeably to the evidence, must have received an enormous reinforcement from the subjacent nitre or combustible merchandise. On the giving way of each floor in succession, the conflagration must have received a reinforcement of deflagrating fuel, so as to have grown rapidly with its growth, and strengthened with its strength. Under these circumstances, the whole of the nitre, becoming liquefied, must have found its way to the cellar. Meanwhile the merchandise and the charcoal of the wood-work must have been conglomerated by the fusibility of the sugar, shell-lac and bitumen, aided by the molasses, and formed thus an antagonistic mass of more than half a million of pounds in weight, deflagrating intensely with the nitre. But whenever, by these means, a portion of the deflagrating congeries attained the fulminating temperature, a detonation must have ensued, causing a temporary lifting of the combustible mass; only, however, to be followed by a more active collision, resulting from the subsequent falling back of the conglomerated combustible mass upon the melted nitre. After every such collision, the combustible congeries must have been blown up to a height augmenting with the temperature, the force of the fall, and extent of reciprocal penetration. The force of the fall would, of course, be as the height. Hence the twelve or thirteen successive detonations indicate as many explosive collisions; while the successive augmentation of the loudness of the reports indicates a proportionable growth of their violence, arising from successively greater elevation and descent.

16. If I am right in supposing that in fulminating power the intensely heated nitre and the combustible merchandise were for equal weights equivalent to gunpowder, if only a sixth of the 300,000 pounds of nitre held in the store was engaged in the final explosion, it would be equivalent to 60,000 pounds of gunpowder.

17. No better way of estimating the force with which the

nitre and combustibles were brought into collision for the last time, at which the finishing explosion took place, has occurred, than that of comparing it with the blow by which nitre and sugar were exploded, as above mentioned, in one of my experiments.

The weight of the combustible matter contained within the store was 700,000 pounds. The store was 90 feet deep by 24 wide. Supposing the horizontal area of the sledge, as applied, to have been $3 \times 3 = 9$ square inches, it seems that for every equivalent horizontal area within the store, there must have been twenty-two pounds, or about three times the weight of the sledge. Hence, in descending from a height of 20 or 30 feet, which there was ample room for it to reach, the combustible congeries may have attained a much greater velocity than could be imparted to the sledge, and may consequently have produced a much more forcible impact. At the same time, this must have caused an intimate penetration and intensity of compression, which by a dead weight it is almost impossible to create.

This explanation, so far as it rests upon the assumption that the combustibles were made to dance upon the surface of the melted nitre, is supported by the fact, that any combustible mass, when thrown upon the surface of incandescent nitre, will undergo a dancing motion, so as sometimes to leap out of a deep pot within which the experiment may be made.

18. The phænomena are not irreconcilable with the idea that some of the earlier explosions arose from the falling of the liquid nitre upon the combustibles before all the floors gave way; but it should be recollected that nitre fuses at a low red heat, and at a cherry-red gives out oxygen gas. The presence of this gas, as well as the deflagration resulting from contact with the liquid nitre, must have caused the floors to be oxidized with a rapidity far exceeding that which takes place during ordinary conflagrations*. To the causes of quick destruction thus suggested, must be added the mechanical force of the explosions directly at war with the persistence of the floors. That, prior to the last explosion, the nitre must have been collected in the cellar, may be assumed from the fact, that the temperature being inevitably far above its fusing-point, the salt must have been all liquefied, and occupying the lowest accessible cavity, on account of its superior specific gravity. This assumption is moreover justified by the circumstance that the force of the explosion appears to have been especially exerted upon the parietes of the cellar,

* See Note to paragraph 9, pages 528, 529.

the walls and surrounding earth having given way in a manner which created astonishment.

19. In order to amplify the practical basis upon which the preceding inferences had been founded, I made some experiments on the combustion of gunpowder in an exhausted receiver, so as to secure the gaseous products evolved. A cylindrical glass receiver, such as is usually employed as a candle-shade, was ground upon a lap-wheel, so as to fit airtight between two discs of sheet brass. The disc for closing the upper opening of the receiver was furnished with two cocks severally, for communicating with an air-pump and barometer gauge. The disc for closing the lower opening of the receiver, so as to form the bottom of the space included, was furnished with an arch of platinum wire soldered to two stouter brass wires, of which one was soldered to the disc, the other secured and insulated in passing through it by a collet of leather, compressed about it by an appropriate screw. These preparations being made, a portion of gunpowder, weighing about twenty-five grains, was so supported on a tray, as to include the middle portion of the platinum wire. The receiver being put into its place, so as to be duly supported by the lower disc and covered by the other, the air was withdrawn, as far as practicable, with a good air-pump. In the next place, the wire was ignited to incandescence. To my surprise, the gunpowder only smoked at first, and did not flash until a perceptible interval had elapsed. When this result ensued, it appeared to be owing to the radiant heat, as the early volatilization of a portion of sulphur had driven the granules away from the wire, so that it did not touch any of them. Subsequently, on allowing the air to enter, and removing the receiver, it appeared that the gunpowder was only partially burned. Thus it became evident that in this way a complete combustion could not be effected. The feebleness of the flash *in vacuo* shows how much confinement is essential to give energy to the explosion of this powerful agent; and its not being forthwith ignited by an incandescent wire, demonstrates, that, as in the case of the apparent quiescence of a globe of volatile matter in an intensely heated cavity, a capability of volatilization delays this process, by preventing the contiguity requisite to a communication of heat.

20. This leads to a discrimination which has not, to the best of my knowledge, been made heretofore. I allude to the difference existing between fulminating *combinations* and fulminating *mixtures*. As an example of the latter, we have gunpowder and other pulverulent mixtures consisting partially of nitre, or chlorate of potash, while, as an exemplifica-

tion of the former, we may advert to aurum or argentum fulminans, or to the fulminates of mercury or silver; also to the chloride or iodide of nitrogen, or perchloric æther. Compounds of the last-mentioned kind, without confinement, break the vessel on which they are exploded. They cannot be used in gunnery, because the force in their immediate vicinity, in proportion to its durability, is too great, so that they burst the chamber before the ball moves an available distance. The elements in these combinations are in a state of intense chemical union, and can only leave that state for another, by which gases and vapours are produced with an instantaneous and almost irresistible expansibility. They require no confinement, because already confined by their reciprocal affinities. In gunpowder and analogous mixtures, the ingredients exist without any forcible coherence, so that an incipient reaction causes a tendency to move apart, which prevents the reaction from extending itself when there is no confinement. This was strikingly shown by attempting to burn *in vacuo* a small cylinder of consolidated gunpowder, made by intense pressure within a metallic tube by a steel piston. This cylinder, about a half-inch in diameter and an inch in length, was placed in contact with a platinum wire within an exhausted receiver. The wire being ignited, a feeble combustion ensued. Subsequent examination showed that the cylinder was only about half deflagrated, the unburnt portion remaining unchanged. It had been extinguished spontaneously, after being completely ignited at the end in contact with the incandescent wire.

21. This was, no doubt, in consequence of the process being effected in a rarefied medium. In order to compare these observations with those which might be made by combustion *in pleno*, I made a larger cylinder of gunpowder, 2 inches in diameter and 2 inches in height, by similar means, and set fire to it by an iron rod ignited at one end. This I caused to touch the top of the cylinder while standing upright at the bottom of a cast-iron pot about 4 inches in diameter, and a foot in depth. The combustion very much resembled that of a rocket, commencing feebly, however, yet terminating with a deflagration so rapid as to be almost explosive. The augmentation of intensity I ascribe to the increased resistance from reaction with the gas evolved, which pressed upon the cylinder with a force like that which elevates a rocket.

22. Finding that, *in vacuo*, a perfect combustion could not be accomplished by the means above mentioned, I resorted to an arrangement through which a cylinder of consolidated gunpowder might be so supported by a rod sliding in

a stuffing-box as to be pushed upwards against a wire ignited by a galvanic battery within an exhausted receiver. When, by these means, the ignition of gunpowder was attempted, it was not very readily accomplished. The part touching the wire appeared to burn feebly; nevertheless, by turning the rod so as to cause the cylinder to revolve, and consequently to be assailed at various points, combustion was induced and gradually extended, and at last completed satisfactorily.

23. The receiver employed was held between two metallic plates, one forming the bottom, the other the cap. Through the middle of the bottom the sliding rod was introduced, so as to be in the axis of the cavity. It was secured by two stuffing-boxes, the object of the outer one being to enable the rod to pass through the orifice of a vessel of oil, employed to prevent the possibility of air entering through that next the cavity. The juncture of the cap with the receiver was covered by cold water, which served to prevent leakage, and keep down the temperature. This was ascertained by a thermometer within the receiver, yet accessible to inspection. The cavity, thus secured against leakage, held 240 cubic inches; the contents being indicated by a column of mercury in a barometer tube situated before a scale graduated into 480 parts. Of course, the whole contents being 240 cubic inches, as above stated, each graduation represented half a cubic inch.

24. The igniting wire was soldered to the ends of brass rods, of which one was soldered to the cap, the other secured by collets of leather, so as to pass through the cap without metallic contact. Consequently, connexion being made between this insulated rod and one pole of a battery, while the other pole had a metallic communication with the cap, the wire might at any moment be made the medium of a circuit competent for its intense ignition.

25. The upper end of the sliding rod supported a little disc of sheet copper, and a little below that disc was supported, in like manner, a larger disc of the same material perforated like a colander.

26. Upon the upper disc, the consolidated gunpowder being supported with all the above-mentioned arrangements, the receiver was replaced.

27. The air was withdrawn until the mercury in the gauge-tube attained nearly the height of the column within an adjoining Torricellian tube, or that of a neighbouring barometer. The height was recorded, likewise the temperature indicated by the thermometer. The fall of the barometrical column of mercury in the gauge-tube, resulting from the operation, was not estimated until the mercury in the thermometer was in

statu quo. The difference in degrees caused in the height of the barometric column, divided by two, gave the number of cubic inches of gaseous matter evolved. This difference was of course set down.

28. In the next place, the temperature being carefully observed and recorded, about 2 cubic inches of a strong solution of caustic potash were added. The consequent absorption, as it declined in rapidity, was assisted by an agitation consequent to moving up and down the rod, and the perforated disc attached to it. When no more absorption could be observed to take place, judging by the quiescence of the mercurial column in the gauge, and when the temperature had returned to the starting-point from which it had been disturbed by the heat generated through the reaction between the alkali and carbonic acid, the height of the column was again recorded, and the difference of degrees, divided by two, was estimated to give the number of cubic inches of carbonic acid generated. Allowance was made for the mechanical effect of the bulk of the alkaline liquid in lowering the mercurial column founded on actual measurement of the effect of a like quantity of water; the mercury being brought to the same height in the gauge-tube, in an experiment made for the purpose with atmospheric air.

29. Three samples of Dupont's powder were obtained from the United States Arsenal, severally designated cannon, musket, and rifle powder. Of each, 75 grains were pressed into an indurated cylindrical mass, as above described, and successively burned in the exhausted receiver.

The following are the results:—

Cannon powder, gas evolved . . .	55 cubic inches.
... absorbed . . .	23 $\frac{1}{2}$...
Musket powder ... evolved . . .	52 ...
... absorbed . . .	21 $\frac{1}{4}$...
Rifle powder ... evolved . . .	51 $\frac{1}{2}$...
... absorbed . . .	20 $\frac{1}{2}$...

Sporting powder, average of two experiments,—

Gas evolved	51 cubic inches.
... absorbed	25 ...

30. As the gas left after the removal of the carbonic acid had all the negative characteristics of nitrogen, it may be concluded from the results above given, that the gaseous products of deflagrated gunpowder consist of nearly equal volumes of carbonic acid and nitrogen.

31. I was naturally led to compare the results of the deflagration of gun-cotton with those of gunpowder. Accordingly, I exposed a tuft of gun-cotton, weighing 25 grains, in the

exhausted receiver in a similar way. I found a retardation in the activity of the combustion arising, as in the case of gunpowder, from the absence of mechanical confinement, diminution of atmospheric pressure tending to lessen the contiguity indispensable to intense chemical reaction.

32. The deflagration of the tuft being effected, it caused an evolution of gas equal to $19\frac{1}{2}$ cubic inches.

33. In order to concentrate the combustible ingredients, resort was had to the apparatus employed in the case of gunpowder, by which means 25 grains of the cotton could be condensed into a cylinder of about half an inch in width, and of a like length.

34. Two specimens of gun-cotton, of the manufacture of Lennig, of 54 grains each, prepared and ignited as above described, gave an evolution equal to $126\frac{1}{2}$ cubic inches.

35. As 75 grains of gunpowder gave only 55 cubic inches of gas at most, it appears that equal weights being employed, gun-cotton causes a gaseous evolution more than three times as great as gunpowder.

36. As 75 grains of gunpowder produce, taking the largest amount in the above table, only 55 cubic inches of gas, it follows, that to produce an effect equal to 54 grains of gun-cotton, $172\frac{1}{2}$ grains of gunpowder would be requisite.

37. The gunpowder evolved little more than seven-tenths of a cubic inch per grain, while the gun-cotton evolved more than two cubic inches per grain.

38. The gas arising from the gun-cotton did not admit of an examination so simple as that given out by gunpowder.

39. By the introduction of 100 cubic inches of oxygen gas, it appeared from the consequent red fumes, and absorption by water, that about 35 cubic inches of nitric oxide had been formed: by the introduction of caustic potash, about 25 cubic inches of carbonic acid were indicated. One-third of the residual gas being exploded with oxygen, appeared to consist of three volumes of hydrogen to four of carbon vapour. The washings gave indications of cyanogen.

40. The coexistence of nitric oxide, carburetted hydrogen and cyanogen, in the products, justifies the idea, that were the heat greater, the expansive effect would be augmented by the transfer of the two atoms of oxygen in the oxide, to the hydrogen and carbon, producing augmentation of temperature, carbonic acid and aqueous vapour. In order to bring the explosive power of gun-cotton to its maximum, I infer that immense resistance would be necessary, thus concentrating and expediting the reaction.

41. The residue of the explosion of gunpowder appears, from a qualitative analysis, to consist of sulphocyanide and

sulphide of potassium, with carbonate and sulphate of potash. The two latter are by much the more abundant products. Probably sulphur is the primary and most energetic ingredient, as when in excess it is, *per se*, known to be capable of completely decomposing potash at a moderate heat, while carbon can only partially effect an analogous change at the highest heat of a furnace. Faraday has recently alleged, that the production of the flame of sulphide of potassium is an important agent in the explosive ignition of gunpowder. It is likely that from the reaction of oxygen with sulphur and potassium, a temperature results sufficiently high for the combustion of the charcoal with oxygen, and of nitrogen with sulphur and carbon, whence ensues carbonic acid and sulphocyanogen, in union with potassium in the one case, and with potash in the other.

42. I have already distinguished the explosion of mixtures like gunpowder from fulminating combinations, of which the constituents, being held together by intense chemical affinity, require no mechanical confinement nor impact to bring or keep them sufficiently near each other for reciprocal reaction. There is, however, another distinction to be made. The explosion of vessels by high steam is altogether the effect of heat and confinement. The resulting violence, when the vessel bursts, is directly as its strength; so that, knowing how many pounds per square inch the vessel was capable of bearing, we know the explosive force to have been exactly equal thereto. But the strength of the containing vessel, in the case of gunpowder, may be very far short of that generated by the gunpowder ignited within it. When held together until the temperature is attained which is requisite for the play of affinities into which the ingredients are disposed to enter, a sudden evolution of heat and gaseous matter takes place, producing a disruptive force far beyond the retaining power of the vessel.

43. Although gun-cotton is a chemical combination consisting of nitric acid and lignin, yet it does not explode, when unconfined, with a violence approaching to that of other fulminating combinations above mentioned. This may be attributed to the fact that neither the elements of nitric acid, nor those of lignin, are held together by a strong affinity, and consequently the forces which resist explosion are but feeble.

Summary.

It is an old and well-accredited maxim in chemistry, to which there are but few exceptions, that fluidity is requisite to chemical reaction. The fluid state, of which the necessity is thus asserted, is with few exceptions attained only through water, or heat, or both. In truth, however, when it is con-

sidered that without heat there could be no fluidity, heat may be viewed as the sole solvent. As respects the induction of the state requisite to chemical reaction, we may consider the solution in which water is the ostensible agent, or igneous fusion in which it is absent, as the only means of bringing the atoms of solids into the state requisite for chemical reaction, through which decompositions and recompositions are effected.

It is well known that the affinities which prevail among the same set of bodies when liquefied by aqueous solution, may be the opposite of those which they exert when indebted to heat solely for liquefaction. Thus there is scarcely any acid which will not displace silicic or boric acid from alkaline bases when in aqueous solution; yet when salts, consisting in part of the most energetic acids, are fused with silicic or boric acid, decomposition ensues in consequence of the union of the acids last-mentioned, with the bases ignited with them.

The sulphates, carbonates, or hydrates of potash, soda, and of some other bases, are *per se* indecomposable at any heat at which their bases cannot be volatilized; yet the nitrates of the same bases are decomposed at the temperature of incandescence. It follows, that if a nitrate be exposed to igneous fusion with any substance consisting more or less of hydrogen, carbon, or sulphur, the oxygen of the nitrate will, by forming water with the hydrogen, carbonic acid with the carbon, or sulphuric acid with the sulphur, cause the nitrate to be replaced by a hydrate, a carbonate, or sulphate*.

But as in every atom of nitrate there are, independently of the base, 5 atoms of oxygen, and since to convert hydrogen into water requires 1 atom, to convert carbon into carbonic acid requires 2 atoms, and to effect an analogous change in sulphur requires 3 atoms, it follows, that for every atom of hydrogen there will be 4 atoms of oxygen liberated, for every atom of carbon 3 atoms, and for every atom of sulphur 2 atoms. Each atom of oxygen is to the weight of nitrate of potash as 8 to 102; hence there will be by hydrogen nearly 32 per cent., by carbon nearly 24 per cent., by sulphur 16 per cent. of oxygen evolved to act upon the excess of the contiguous combustible matter. Meanwhile it must be recollected that in gum, sugar, starch, and lignin (or fibre of wood, cotton, or linen), both hydrogen and oxygen exist in due propor-

* The power of decomposing incandescent nitre by aqueous vapour, which was inferred by me to exist in 1845, has since been fully verified by the employment of this vapour by an American chemist, Tilghman, to effect the decomposition of compounds containing potash, or other alkaline bases capable of forming hydrates, *per se*, indecomposable by heat. (See Note, pages 531, 532.)

tion to generate water; and besides these compounds formed with oxygen, we have nitrogen to aid*, which is more incoercible than water or carbonic acid. Since at the heat produced by the combustion of hydrogen or carbon, with pure oxygen, iron, the most tenacious of all the materials at our command, is perfectly fusible, it is evident that by mechanism we cannot restrain the expansive force of the gaseous products producible as above represented. I believe I may say, that water has never been confined under a white heat. Yet the expansive force of liquid carbonic acid is at the freezing-point of water 36 times as great as the pressure of this liquid at its boiling-point. It has already been observed, that nitrogen in expansive violence must go beyond carbonic acid. It follows, that excepting the blow of a hammer, or the force created by gravitation in falling bodies, we have no means by which we can enable nitre, in the state of incandescent igneous fluidity, to come into close contact, *even for an instant*, with masses of combustible matter, like those which it was made to encounter in the store of Messrs. Crocker and Warren.

It is to be presumed that it has been the want of this force which has caused efforts to produce explosions between nitre and combustibles to fail; and it is to the presence of this force, where the fall of enormous masses of agglutinated combustible matter upon incandescent liquefied nitre may be reiterated, that I ascribe the destructive explosions, which, under such circumstances, have been so prolific of impoverishment, mutilation, and death.

LXXIII. *Intelligence and Miscellaneous Articles.*

PREPARATION OF ATROPIA BY MEANS OF CHLOROFORM.

BY M. RABOURDIN.

THE following is the process proposed by the author above named: —Take fresh belladonna as soon as it begins to flower; bruise it in a marble mortar and press out the juice, which is to be heated to 80° or 90° centigrade to coagulate the albumen. When the juice thus clarified is cold, add to every litre 4 grammes of caustic potash and 30 grammes of chloroform; shake the whole for a minute, and let it stand. In half an hour, the chloroform holding atropia is deposited, having the appearance of a greenish oil; the supernatant liquor is poured off, and replaced by a little water; this is afterwards poured off, and the washing is to be continued till the water comes away limpid. The chloroform solution is then to be put into a small tubulated retort, and distilled on a water-bath until all the chloroform has passed into the receiver. The residue in the retort is to be treated with a little water, acidified with sulphuric acid, which dissolves the atropia, and leaves a green resinoid matter; the filtered

* Nitric acid consists of 1 atom of nitrogen as well as 5 of oxygen.

solution is colourless. To have the atropia pure, it is only requisite to pour into the acid solution a slight excess of solution of carbonate of potash, and to dissolve the precipitate in rectified alcohol. This solution yields, by spontaneous evaporation, fine groups of acicular crystals of atropia.

In the absence of the fresh plant, the officinal extract, which has been well prepared, may be substituted for it: 30 grammes of extract of belladonna, prepared from the purified juice of the plant, were dissolved in 100 grammes of distilled water; to the filtered solution were added 2 grammes of caustic potash and 15 grammes of chloroform. After agitating the mixture for a minute and setting it aside for half an hour, the chloroform containing atropia was deposited, the supernatant liquor was decanted and replaced by water, which was three times renewed; the chloroformic solution weighed 11 grammes, so that 4 grammes of chloroform were lost during the manipulation. This solution exposed to the air rapidly evaporated, leaving a crystalline greenish mass consisting almost entirely of atropia; this, treated with dilute sulphuric acid, and precipitated by carbonate of potash, gave a precipitate weighing 16 centigrammes. It was totally soluble in rectified alcohol, and yielded by spontaneous evaporation fine needles of atropia.

The author of this mode of operating with belladonna is of opinion that it is susceptible of generalization, and of application to many other substances containing organic alkalies; if it does not prove an economical method of preparing these products, it will serve, at any rate, in some cases, as a ready means of estimating the value of certain commercial products.

In a future communication the author proposes to give a process for estimating quickly and commercially the alkalies of cinchonas, by acting on very small quantities of them; and will also show that by means of chloroform, traces of iodine may be ascertained more advantageously than by starch.—*Comptes Rendus*, Octobre 14, 1850.

ON THE COMPOSITION OF CERTAIN NATURAL ORGANIC BASES.

BY M. A. DE PLANTA.

It is remarked by the author, that the researches of a great number of chemists have of late years given great impulse to the study of the composition of the vegetable alkalies. In this so cultivated a field there remain, however, some gaps to be filled, and this M. de Planta has, therefore, endeavoured to effect with respect to atropia, daturina and aconitina.

Atropia.—This was prepared by M. Merck of Darmstadt; it had the form of very fine needles, which were unalterable by exposure to the air, and heavier than water. At common temperatures, 1 part of atropia requires about 300 parts of water for solution. Alcohol dissolves it in all proportions; æther less readily.

At 90° centigrade, atropia melts into a colourless transparent liquid, and becomes on cooling a brittle mass, in which, after long-continued fusion, crystals are often observable grouped in stars. At 140° it is partly volatilized, but the greater portion is decomposed. Heated on a strip of platina, it readily fuses, melts, swells up and

emits white fumes, and afterwards burns with a brilliant flame, leaving a black, bright charcoal.

The aqueous solution of atropia possesses strong alkaline reaction ; it combines with alkalies [acids?] to form neutral uncrystallizable salts. Evaporated *in vacuo*, these salts become of the consistence of syrups ; they dissolve very readily in water and in alcohol, less so in æther. The hydrochlorate of atropia acts with reagents in the following manner :—Potash, ammonia and carbonate of potash, added to very concentrated solutions of this salt, give pulverulent precipitates readily soluble in excess of the reagent. Carbonate of ammonia, bicarbonate of soda, and phosphate of soda, do not give precipitates. Chloride of gold gives a crystalline precipitate of a yellow colour, which is but slightly soluble in hydrochloric acid. Chloride of platina forms a pulverulent precipitate, which readily agglutinates into a resinous mass, dissolving in hydrochloric acid. The bichloride of mercury precipitates only very concentrated solutions of hydrochlorate of atropia. The double iodide of mercury and potassium forms a white thick precipitate, which agglutinates strongly on the addition of hydrochloric acid. The iodide of potassium and sulphocyanide of potassium give no precipitate. Tincture of iodine precipitates the hydrochlorate of atropia of a brown colour ; iodic acid does not colour it ; neither infusion nor tincture of galls yields a precipitate till hydrochloric acid is added.

Picric acid occasions a sulphur-yellow precipitate, and nitric acid does not dissolve it.

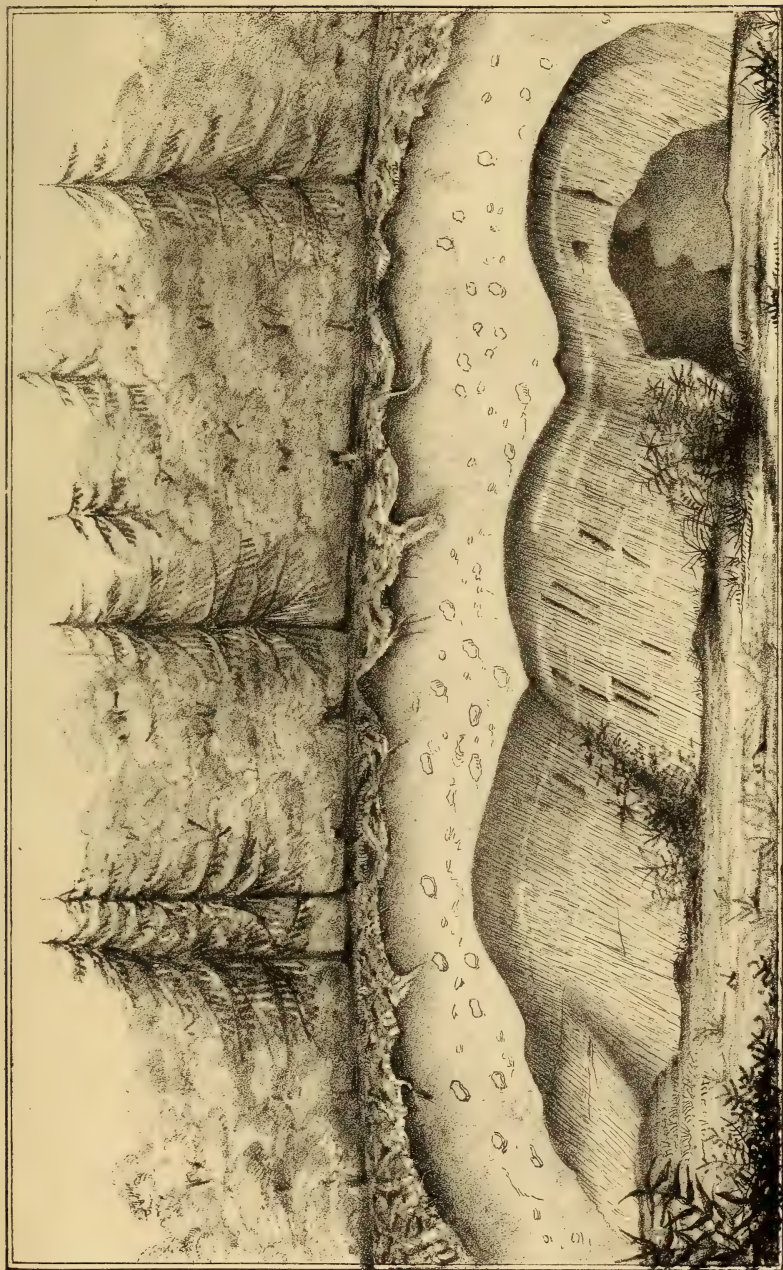
M. Planta analysed uncombined atropia and the double chloride of atropia and gold. His analyses indicate the composition as $C^{34} H^{23} NO^6, HCl + Au Cl^3$; the composition of uncombined atropia is consequently expressed by $C^{34} H^{23} NO^6$. This formula was checked by an experiment, by which the author determined the quantity of hydrochloric acid gas absorbed by a given weight of atropia. He found that 100 parts of atropia absorbed 13·85 parts of hydrochloric gas, which agrees very well with the equivalent deduced from the salt of gold. M. Planta also ascertained the quantity of sulphuric acid exactly necessary to saturate a given quantity of atropia. The equivalent calculated by this last experiment agreed equally well with the theoretical equivalent.

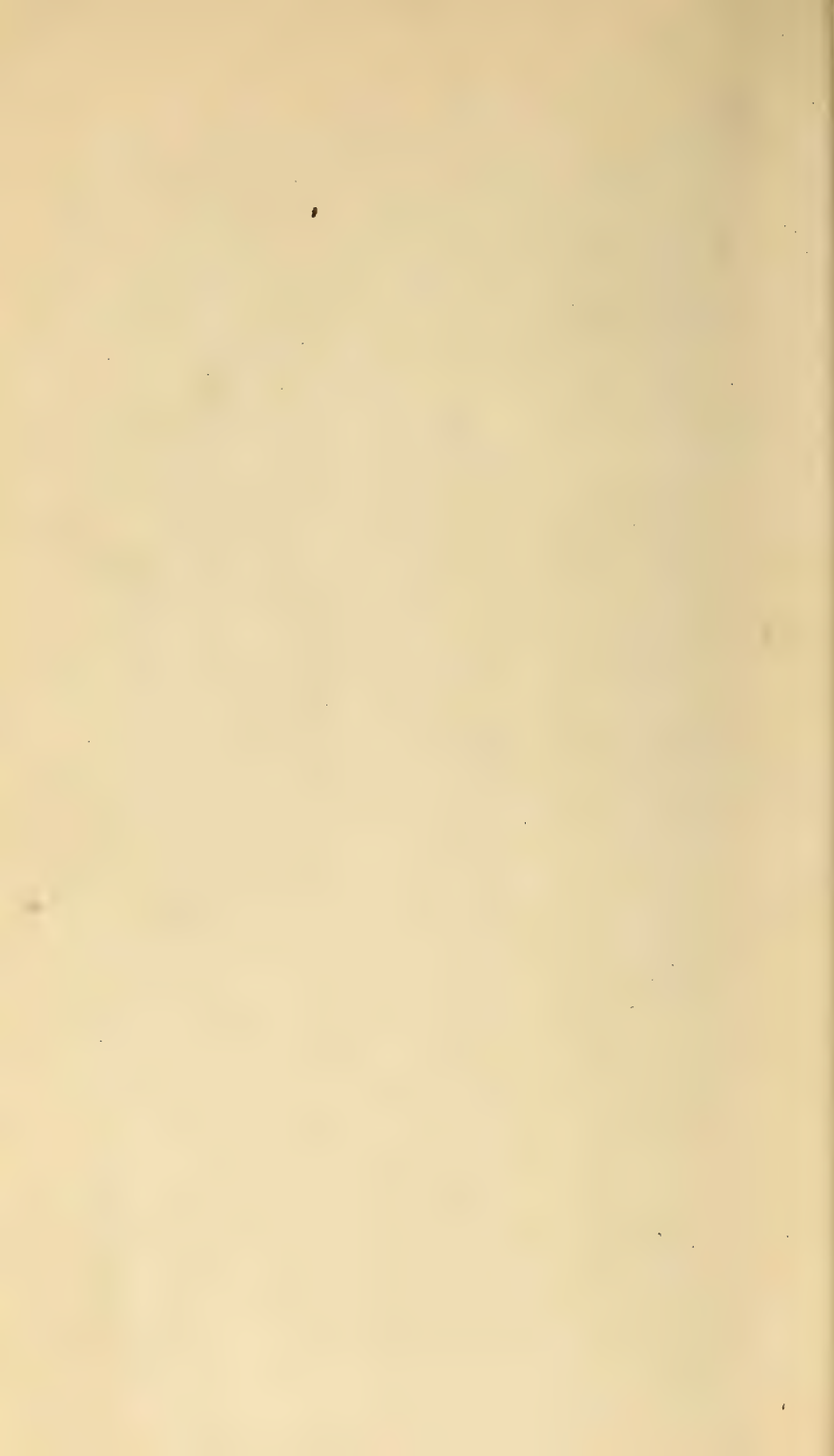
Daturina.—This was extracted in 1833 by MM. Geiger and Hesse from the *Datura stramonium* ; it has the form of small brilliant needles aggregated in tufts ; it is colourless, unalterable in the air, and heavier than water.

M. Planta has observed, that in its combination of properties, and also in its composition, this alkali is similar to atropia. The double chloride of daturina and gold may be obtained under the form of a crystalline mass, of a fine golden-yellow colour.

Its composition is expressed by the formula $C^{34} H^{23} NO^6, HCl + Au Cl^3$.

Aconitina.—The discovery of this base is also due to MM. Geiger and Hesse, who obtained it in 1833 from the *Aconitum napellus*. Previously to examining the properties and composition of this base, M. Planta purified the commercial article by the following process :





Crest of wave

.2433

.1753

.1153

.0663

.0299

se.

m of Channel.

Crest of wave

6178

6089

5745

5241

4613

3906

3164

2433

1753

1153

6663

0299

Level of Repose.

Bottom of Channel.



after dissolving it in æther, he evaporated the æthereal solution; the syrupy residue was redissolved in absolute alcohol, and the solution was gradually poured into cold water, with continual shaking. There was thus obtained a dense flocculent precipitate, which was pressed between folds of paper and dried *in vacuo*.

Aconitina thus prepared has the form of a powder, which is colourless and inodorous, and completely unalterable in the air. Heated on a strip of platina, it readily melts, takes fire and leaves a coaly residue, which easily incinerates. It is impossible to volatilize it partially, like atropia; it is heavier than water, and is very slightly soluble in it. It dissolves very readily in alcohol, and less so in æther.

It fuses at 80° , and on cooling becomes a vitreous, transparent mass. At 120° it begins to become brown.

It has a strong alkaline reaction, and saturates acids perfectly. Its salts are uncrystallizable. Hydrochlorate of aconitina reacts as follows:—Potash, ammonia and carbonate of potash, form a white flocculent precipitate of aconitina, slightly soluble in excess of the reagent. Carbonate of ammonia, bicarbonate of soda, and phosphate of soda give no precipitate. Chloride of gold gives a thick precipitate of a whitish yellow colour, slightly soluble in hydrochloric acid. With the chloride of mercury and the sulphocyanide of potassium, white cheesy precipitates are obtained, and with tincture of iodine a kermes-coloured precipitate. Tincture of galls and gallic acid precipitated aconitina after the addition of a drop of hydrochloric acid. Picric acid forms a dense precipitate of a sulphur-yellow colour.

The author's analyses indicate as the formula of aconitina $C^{60}H^{47}NO^{14}$; the double hydrochlorate of aconitina and gold contains $C^{60}H^{47}NO^{14}, HCl + AuCl^3 + H^2O^2$.

M. Planta found that 0.2970 gr. of this alkali absorbed at 100° , 0.0460 of hydrochloric gas, which corresponds to 31.41 of hydrochloric acid in 100 parts of the hydrochlorate represented by the formula $C^{60}H^{47}NO^{14}, 2HCl$.—*Journ. de Chim. Méd.*, Octobre 1850.

MAGNETIC AND DIAMAGNETIC CONDITION OF GASES.

Nov. 29, 1850.

The Bakerian Lecture was delivered yesterday by Prof. Faraday to a crowded audience. At this late period of the month we can only glance at the highly interesting investigations laid before the Royal Society, reserving a fuller notice for our next Number.

One of the conclusions arrived at by the author was, that the motions of magnetic and diamagnetic bodies in each other do not appear to resemble those of attraction or repulsion of the ordinary kind, but to be of a differential action, dependent perhaps upon the manner in which the lines of magnetic force were affected in passing from one to the other during their course from pole to pole, the differential action being in ordinary cases between the body experimented with and the medium surrounding it and the poles. A method of showing this action with the gases is described, in which delicate soap-bubbles are made to contain a given gas, and then, when held in the magnetic field, approach, or are driven further off, according as they contain gases, magnetic or diamagnetic, in relation to air. Oxygen passes inwards or tends towards the magnetic axis, confirming the results formerly described by the author.

Perceiving that if two like bubbles were set on opposite sides

of a magnetic core or keeper cut into the shape of an hour-glass, they would compensate each other, both for their own diamagnetic matter and for the air which they would displace; and that only the contents of the bulbs would be virtually in a differential relation to each other, the author passed from bubbles of soapy water to others of glass; and then constructed a differential torsion balance to which these could be attached, of the following nature:—A horizontal lever was suspended by cocoon silk, and at right angles to the end of one arm was attached a horizontal cross-bar, on which, at about $1\frac{1}{2}$ inch apart, and equidistant from the horizontal lever, were suspended the glass bubbles; and then the whole being adjusted so that one bubble should be on one side of the iron core and the other on the other side, any difference in their tendency to set inwards or outwards from the axial line causes them to take up their places of rest at different distances from the magnetic axis; and the power necessary to bring them to an equidistant position becomes a measure of their relative magnetic or diamagnetic force.

In the first place, different gases were tried against each other, and when oxygen was one of them it went inwards, driving every other outwards. The other gases, when compared together, gave nearly equal results, and require a more delicate and finished balance to measure and determine the amount of their respective forces.

The author now conceived that he had attained to the long-sought power of examining gaseous bodies in relation to the effects of heat and the effects of expansion separately; and proceeded to an investigation of the latter point. For this purpose he prepared glass bubbles containing a full atmosphere, or half an atmosphere, or any other proportion of a given gas; having thus the power of diluting it without the addition of any other body. The effect was most striking. When nitrogen and oxygen bubbles were put into the balance, each at one atmosphere, the oxygen drove the nitrogen out powerfully. When the oxygen bubble was replaced by other bubbles containing oxygen, the tendency inwards of the oxygen was less powerful; and when what may be called an oxygen vacuum (being a bulb filled with oxygen, exhausted, and then hermetically sealed) was put up, it simply balanced the nitrogen bubble. Oxygen at half an atmosphere was less magnetic than that at one atmosphere, but more magnetic than other oxygen at one-third of an atmosphere; and that at one-third surpassed the vacuum. In fact, the bubble with its contents was more magnetic in proportion to the oxygen it contained. On the other hand, nitrogen showed no difference of this kind; whether a bubble contained that gas more or less condensed, its power was the same. Other gases (excepting olefiant and cyanogen) seemed in this first rough apparatus to be in the same condition.

Hence the author decides upon the place for zero, and concludes that simple space presents that case. When matter is added to space it carries its own property with it there, adding either magnetic or diamagnetic force to the space so occupied in proportion to the quantity of matter employed; and now thinking that the point of zero is well determined, he concludes to use the word magnetic as a general term, and distinguish the two classes of magnetic bodies into paramagnetic and diamagnetic substances.

* * This notice has been substituted for two articles which are noticed in the table of contents.

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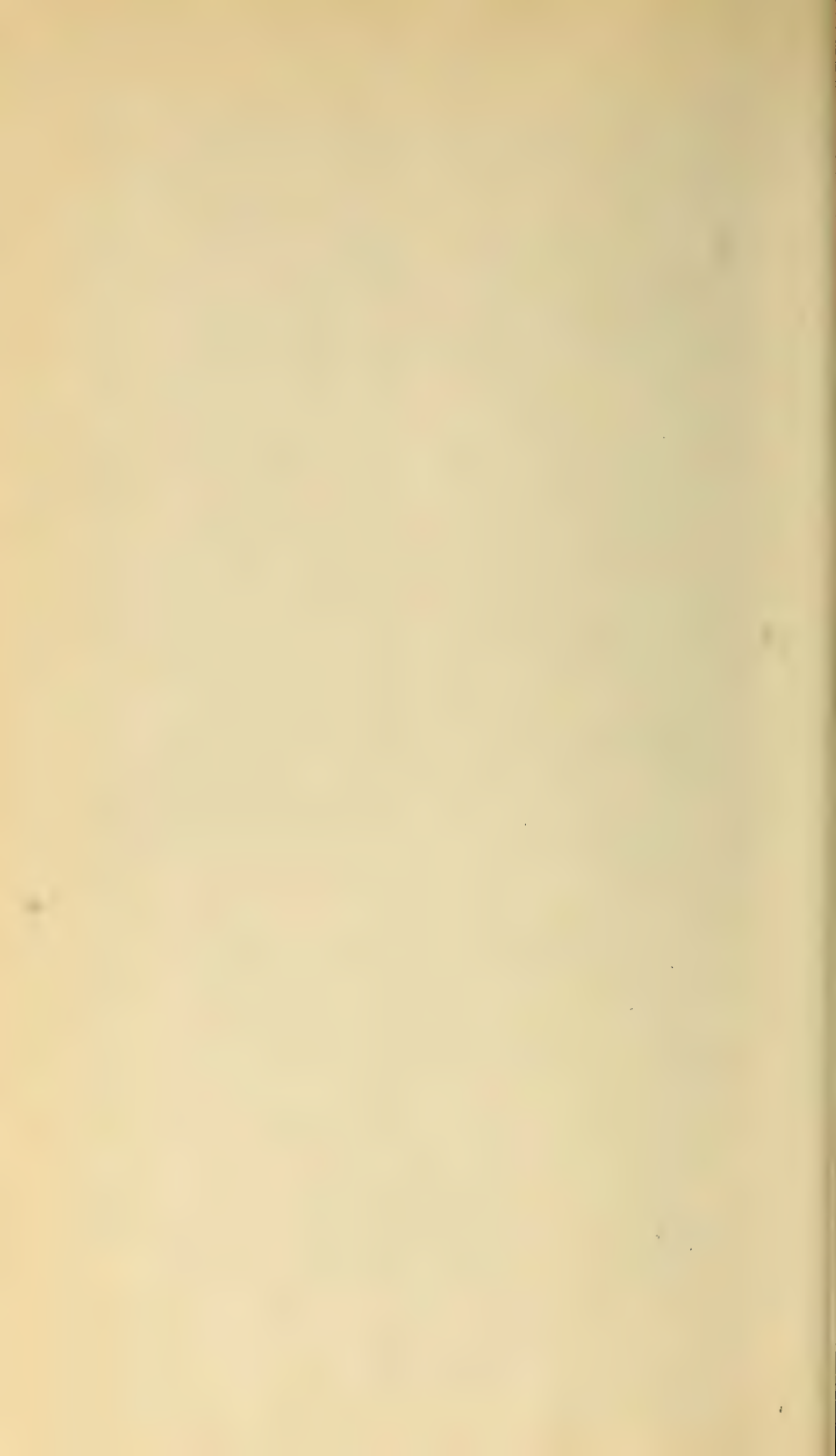
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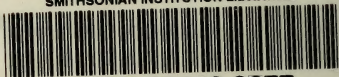
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